# DR S. J. F. PHILPOTT'S WAVE-THEORY

BY LEWIS F. RICHARDSON

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## I. INTRODUCTION

For the past twenty-two years Dr Philpott has conducted researches on the fluctuations in human output when routine tasks are performed. Together with his students he has accumulated a great store of experimental results, which must surely be valuable for various purposes, whatever one may think of a particular interpretation.

In various conversations I have heard and expressed general vague incredulity about Dr Philpott's wave-theory. However, remembering that the most unfair type of criticism is that which is so vague that it cannot be answered, I shall now try to dissect my incredulity, in order to emphasize precise objections, and to blow away what was misunderstanding or mere chaff.

The most startling of Dr Philpott's assertions is his claim to have measured $4{\cdot}076 \times 10^{-23}$ sec. by experiments on the time variations of the rate at which people perform routine mental tasks such as arithmetic. This is not a new nor a passing claim. It appears in his Monograph of 1932 (13) and again in his paper dated 1950 (15). In that interval of 18 years there has been no revision of the significant digits, such as usually follows the first measurement of a physical constant. He calls this remarkable time $T_0$; and it will be so denoted here. In the 1950 paper $T_0$ is compared with various constants in atomic physics. There are two surprising features about $T_0$: the alleged determination of a possibly electronic constant in a psychological laboratory, and the enormous ratio of $10^{-23}$ sec. to the least time which one can estimate mentally, say $10^{-1}$ sec. Let us consider these features in turn.

## II. PROFESSIONAL DEPARTMENTS

We are accustomed to regard psychology and physics as separate disciplines. Their separation is a matter of social convenience, an affair of salaries and appointments. Below the level of social organization there is no reason to expect psychology to be detached from physics. It is generally believed that arithmetic somehow depends on the brain, and that the brain contains electrons. So it would be rash to declare that the speed of working arithmetic could not possibly depend on electronic constants. The objection to this part of Dr Philpott's claim seems to me to resemble a gardener's objection to a potato plant coming up among his cabbages.

## III. EXPERIMENTAL PRECISION

Dr Philpott's apparatus was quite ordinary. It consisted of paper, print, pencils and a chronograph. The determination of any ratio to a large number of significant digits usually requires elaborately refined apparatus and technique. Dr Philpott states his

remarkable number as $4{\cdot}076 \times 10^{-23}$ sec. I assume that he would be willing to jettiso the ·076 if he could convince psychologists about the 4; and that what we mainly ha to discuss is the $10^{-23}$.

## IV. The estimation of large ratios

Let us next consider whether very large ratios such as $10^{-23}$ have ever been measured, least roughly, by means of crude apparatus.

Dr G. F. C. Searle, F.R.S., used to show his students how to measure the wave-leng of light, about $6 \times 10^{-5}$ cm., with a candle and a pocket handkerchief. The result w convincing, because a century of research with diverse and better apparatus had establishe the wave-theory of light.

In order to notice pioneer measurements made with crude apparatus one should searc in out-of-date publications. A notable example is Loschmidt's estimate of the diamet of molecules which he published in 1865. The measurements were of the diffusivity gases and of the ratio of the volume of a gas to that of its liquid, both of them simple b modern standards of experimentation. The theory was the kinetic theory of gases (s Winkelmann (31)).

Another early estimate of the diameter of a molecule, about $10^{-7}$ cm., was published b Lord Rayleigh in 1890 (see Poynting & Thomson (20)). Rayleigh dropped shavings camphor on to a water surface which had been contaminated by a small and measur amount of oil. Then by merely cubing $10^{-7}$ cm. and multiplying by the density of o one can estimate the mass of a molecule of oil to be of the order of $10^{-21}$ g. The mode value for oleic acid is $0{\cdot}469 \times 10^{-21}$ g.

From these examples we may conclude that to make a rough estimate of a very extr ordinary number, such as Dr Philpott's $10^{-23}$ sec., it is not always necessary to ha elaborate apparatus; the indispensable necessity is trustworthy theory.

## V. The notion of strict periodicity on the scale of log time

What is the theory in which Dr Philpott places so much trust? It is his belief in t existence of waves of constant period on the scale of $\log_{10}$ time. And how did this bel arise? He first noticed such waves in the observations themselves. Afterwards he soug for collateral evidence, and claims to have found some in the rate of evolution of hydrog from zinc in acid. That might be distantly relevant, for there are electrochemical reactio in nerves. He also claims affinity with the log time in E. A. Milne's astronomy; but th analogy is, I suggest, altogether too far-fetched. Dr Philpott does not mention any oth human periodicities in the scale of log time. But, if we may seek guidance from periodici in ordinary time, there are many human alternations, such as sleeping and wakin breathing, the heart-beat, and the alpha-rhythm shown by the encephalogram. Each these has a frequency which wanders a bit. In B. van der Pol's (18) nomenclature they a probably 'relaxation-oscillations', which differ from pendular oscillations in being l strictly periodic. For emphasis my chief objections are numbered and restated.

*Objection I.* As all other human behaviour is somewhat irregular, ought we to imagi that mental waves in log time would be periodic, quite strictly?

## VI. EXTRAPOLATION

There is a statistical maxim that, when the theory is derived only from the observations themselves, interpolation is usually safe, but extrapolation is usually risky. This maxim was strikingly illustrated, in its application to regression formulae, by Emily Perrin (11) in a paper called 'The dangers of extrapolation'. It is desirable therefore to be clear as to whether $T_0$ lies between Dr Philpott's observations or outside their range. Nominally the observational range was from 0 to 1000 sec., and $10^{-23}$ sec. lies within that nominal range. But actually an experiment could not be started with an accuracy better than 0·1 sec., and the first observational datum was the amount of the task accomplished during the first 5 sec., which may suitably be concentrated at 2·5 sec. Therefore $T_0$ was obtained by extrapolation. To see whether the extrapolation was long or short we must remember that it was done on the scale of log time.

| | | | | |
|---|---|---|---|---|
| The times in seconds were | $4 \times 10^{-23}$ | | 2·5 | 1000 |
| Their logarithms are respectively | −22·4 | | 0·4 | 3 |
| The differences of these logarithms are | | 22·8 | 2·6 | |

This very long overhang is another of my chief objections.

*Objection II.* Extrapolation over a range as long as 22·8 from observations made in a range as short as 2·6, by way of a law inferred only from those observations themselves, is far too risky.

## VII. A TEST FOR RANDOMNESS

A grand-total work curve, actually a polygon, which appeared in Dr Philpott's Monograph ((13), p. 97), has been selected by him recently ((14), p. 124) as representative. It runs for 300 sec. and therefore presumably is constructed from 60 successive intervals of 5 sec. each. M. G. Kendall ((9), p. 124, art. 21·43) gives a very simple test to decide whether such a polygon is drawn from a mere random assortment of disconnected ordinates. Let there be $N$ ordinates, so that here $N=60$. Let $p$ be the number of crests and troughs, counting both. If there is nothing systematic about the ordinates, $p$ may, for finite $N$, be taken as normally distributed about the mean $\frac{2}{3}(N-2)$ with variance $(16N-29)/90$. Insertion of $N=60$ gives mean = 38·7, variance = 10·34; therefore standard deviation = 3·216. On the diagram $p=33$. This differs from the mean by 5·7, which is 1·77 times the standard deviation.

*Objection III.* This overall test leaves one with the impression that the spikiness of the 'grand-total work-curve' may be mostly random, and that to search among it for periodicity may be fruitless.

## VIII. THE ASCERTAINMENT OF OBSERVED PERIODS

Let us next consider the alleged waves in the observed, as distinct from the extrapolated, range of log time. A great store of fluctuations exists in the meteorological records of pressure, temperature, wind, rainfall and humidity. Some of these fluctuations are conspicuous and understandable, being daily and annual variations. Other meteorological fluctuations are as obscure, complicated and mysterious as those in individual work-curves. The history of meteorological research may therefore provide some useful hints.

Sir Gilbert Walker, F.R.S. (29), who paid special attention to periodicity and was lookin back over his long experience of meteorology, wrote in 1936 that probably 'ninety-fiv per cent of the periods announced are non-existent'. This experience suggests that the must be something seductive about the idea of periodicity, which leads even carefu researchers to see periods which are not present. When the fluctuations are as complicate as work-curves, the method of spotting troughs by eye is quite discredited. Yet Dr Philpot as recently as 1949, was alluding to it as though it were respectable ((14), Fig. 1 Personally I find his visually spotted periods quite unconvincing; but I cannot numb that as a serious objection, for my visual judgement may be as untrustworthy as hi M. G. Kendall ((9), p. 402) remarked that 'Experience seems to indicate that few thing are more likely to mislead in the theory of oscillatory series than attempts to determi the nature of the oscillatory movement by mere contemplation of the series itself'.

The respectable methods for this purpose are the Schuster periodogram and the metho of serial correlation. Both are laborious computations. Strictly periodic waves suit th periodogram. Dr Philpott has long since toiled at both these methods and has publishe considerable results in his Monograph ((13), pp. 87–96).

The interpretation of the periodogram requires a test of statistical significance. Suc tests are available (Walker (28), Kendall (9)). But I cannot find any in Philpott's wor Nor are his periodograms published in such a manner that the reader can apply the te himself.

*Objection IV.* The strictly periodic waves which Dr Philpott claims to have observe were first noticed by a discredited method and were afterwards investigated by a reliab method which, however, stopped just short of a decisive conclusion.

## IX. The method of extrapolation

Dr Philpott habitually writes about waves without specifying the wave-form. This w not do for precise argument. For in popular speech there are sea-waves, hair-wave heat-waves; almost anything that moves to and fro continuously is called a wave. Amo precisely defined curves the Bessel function $J_0(x)$ would be called a wave, yet it is n a periodic function. From the context it may be gathered that Dr Philpott is probab thinking about sines or cosines; so for definiteness I shall translate his argument in cosines.

For this purpose it is necessary to accept, at least temporarily, Dr Philpott's idea th the periodicities on the scale of log time are strict. Accordingly, let my objectio I, II, III, IV now be put away into cold storage while we look at the matter from anoth point of view. Let $t$ be the time in seconds, and let the experiment begin at $t=0$. He reli chiefly on 'grand-total curves' formed by adding the ordinates of the individual wor curves of large numbers of persons. He avers that the grand-total curve is the sum waves, that the wave-lengths are integral multiples of 0·0016 on the scale of $\log_{10}$ (I have not found any convincing reason for that number 0·0016; but let it pass for t present.) He begins by assuming (e.g. (14), p. 129) that there is a time $T_0$ at which all wav have a common trough point. From the context it appears that a 'trough point' another name for a minimum. The assumption that $T_0$ exists is permissible, because, this stage, the numerical value of $T_0$ is left undetermined.

To argue by easy steps, we may note that $-\cos(\log_{10} t)$ has minima at $\log_{10} t = 0, 2\pi, 4$

and in general at $2s\pi$, where $s$ is an integer positive, zero, or negative. Then $-\cos(2\pi \log_{10} t)$ has minima at $\log_{10} t = 0, 1, 2, 3, \ldots, s$. And $-\cos\left\{\frac{2\pi \log_{10} t}{0{\cdot}0016n}\right\}$ has minima at $\log_{10} t = 0{\cdot}0016n$, $0{\cdot}0032n$, $0{\cdot}0048n$, and in general at $0{\cdot}0016sn$.

It will be assumed, on Dr Philpott's authority, that $n$ is a positive integer. Further it is plain that

$$-\cos\left\{\frac{2\pi\,(\log_{10} t - \log_{10} T_0)}{0{\cdot}0016n}\right\} \text{ has minima at } (\log_{10} t - \log_{10} T_0) = 0{\cdot}0016n,\ 0{\cdot}0032n,$$
$$0{\cdot}0048n, \text{ and in general at } 0{\cdot}0016sn. \qquad (1)$$

Dr Philpott assumes that such waves are occurring simultaneously for various integers $n$, and that the work-curve is found by adding their ordinates. He does not specify their amplitudes. For definiteness I insert an amplitude $A_n$ which is constant in time, and, to prevent troughs changing into crests, must be positive. The sum of the waves is accordingly

$$A_0 + \sum_{n=1}^{\infty} -A_n \cos\left\{\frac{2\pi\,(\log_{10} t - \log_{10} T_0)}{0{\cdot}0016n}\right\} = \phi(t) \quad \text{say.} \qquad (2)$$

This function $\phi(t)$ is supposed to represent the ordinate of the 'grand-total work-curve'. As the ordinate of the grand-total work-curve is finite, the sequence $A_1, A_2, A_3, \ldots, A_n$, must be such that the series (2) converges as $n \to \infty$. This will happen provided $\Sigma A_n$ converges. There are evidently abundant possibilities of convergence, so that we need not stop to consider them in detail.

In a Fourier series $n$ is in the numerator of the angle, so that the wave-lengths of the terms become shorter as $n$ increases. In (2), which may suitably be called Philpott's series, $n$ is in the denominator of the angle, so that, as $n$ increases, the wave-length of the term becomes longer.

Dr Philpott is much impressed by an observed minimum of his grand-total work-curve at $t = 900$ sec. (Personally I do not find it conspicuous; but let that also pass for the present.) He wished therefore to choose the numerical value of $T_0$ so that plenty of the individual waves in his series (2) should have minima at $t = 900$ sec. By trial he found that $T_0 = 4{\cdot}076 \times 10^{-23}$ sec. gave a remarkable crop of minima at $t = 900$ sec. Let us verify the arithmetic. The coefficient of $A_n$ in (2) will be a minimum if, and only if,

$$\frac{\log_{10} t - \log_{10} T_0}{0{\cdot}0016n}$$

is an integer, positive, zero, or negative. (3)

Now $$\frac{\log_{10} 900 - \log_{10} (4{\cdot}076 \times 10^{-23})}{0{\cdot}0016n} = \frac{25{\cdot}3440083}{0{\cdot}0016n} = 15840{\cdot}0_0/n.$$

When expressed in its prime factors, $15840 = 2^5 \times 3^2 \times 5 \times 11$. Therefore $15840/n$ is an integer for $n = 1, 2, 3, 4, 5, 6, 8, 9, 10, 11, 12$ and for many larger $n$. But $15840/n$ is not an integer for $n = 7, 13, 14, 17, 19, 21, 23, 25, 26, 27, 28, 29, 31$, and many larger $n$. The lowest exception, namely, that for $n = 7$, especially spoils the beauty of the scheme. But we can easily remedy that. Let $T_0$ be determined so that

$$\frac{\log_{10} 900 - \log_{10} T_0}{0{\cdot}0016n} = \frac{7 \times 15840}{n}.$$

This gives $T_0 = 10^{-175} \times 3{\cdot}517$ sec.

It is easy to discover marvellous numbers by such methods!

It is reasonable to ask, however, whether a more ordinary $T_0$ could satisfy Dr Philpott requirements? Or whether he has obtained plenty of coincident minima in the neate possible manner? Let us suppose that it would suffice if the first $n$ of the elementa waves in (2) had minima at $t = 900$ sec.; and let us determine $T_0$ for successively increasi $n$ by means of the condition (3). Let $L_n$ denote the least common multiple of the first whole numbers. Then the condition (3) becomes

$$(\log_{10} 900 - \log_{10} T_0)/0{\cdot}0016 = L_n.$$

When solved for $T_0$ this gives

$$T_0 = 900 \times 10^{-0{\cdot}0016 L_n}.$$

By obvious arithmetic I find from (5) that $T_0$ runs thus as a function of $n$:

| $n$ | 1 | 5 or 6 | 7 | 8 | 9 or 10 | 11 or 12 | 13 or 14 or 15 |
|---|---|---|---|---|---|---|---|
| $L_n$ | 1 | 60 | 420 | 840 | 2520 | 27720 | 360360 |
| $T_0$ (sec.) | 896·7 | 721·5 | 191·5 | 40·76 | 0·0836 | $10^{-42} \times 4{\cdot}0017$ | $10^{-574} \times 2{\cdot}389$ |

Any observational uncertainty about 0·0016, when multiplied by 27720, or by Dr Philpott 15840, would cause a considerable alteration in $T_0$. This is quite a minor objection, aim merely at so many as four significant digits in his 4·076.

As thus determined, $T_0$ is the *greatest* time that will give coincident minima at $t = 900$ s for the first $n$ of the waves. For example, Dr Philpott's $4{\cdot}076 \times 10^{-23}$ sec. does not gi a minimum for the seventh wave, and so, from that restricted point of view, is no bett than 721·5 sec.

*Objection V.* There are many widely different numerical values of $T_0$ that seem reasonab likely to give a minimum of 'Philpott's series' (2) at any particular time, such, f example, as $t = 900$ sec.

It may be said that this objection is unfair, because Dr Philpott was considering n merely a single trough, but several sequences of troughs; and because he has publish a comprehensive discussion of sequences in Appendix B of his Monograph (13). I do n understand that Appendix. Anyway the theory of $T_0$ is very complicated and uncle Moreover, as soon as sequences of observed troughs are mentioned, my suppress objections III and IV rush out of their prison, clamorously protesting.

## X. Conclusions about $T_0$

The above objections numbered I, II, III, IV, V compel me to reject the findi $4{\cdot}076 \times 10^{-23}$ sec. It has been derived from the observations by methods of computati that are not at all trustworthy. Nor is any moderate amendment in sight that co rescue either the theory by which $T_0$ was obtained, or the numerical value of $T_0$. To lo for any physical explanation of $T_0$ is therefore a waste of intellect. I must plead guilty having stimulated the search for physical interpretations some ten or fifteen years a by suggesting, in a letter to Dr Philpott, that $T_0$ was the time taken by light to go rou the equator of an electron. That remark was made in jest, but unfortunately it has be taken seriously. The proper treatment of $T_0$ would be to consign it to oblivion; but af so much publication that will now be difficult. I am sorry to find myself in stro disagreement, about $T_0$, with one of my former teachers, for whom otherwise I share the general high respect.

## XI. SINGLE CURVES

It would indeed be tragic if no conclusions were to emerge from a study of 700 work-curves. Although, as I find, the grand totals contain little of interest, it may well be that the individual curves are most instructive. From the titles of theses deposited in the University of London Library by Dr Philpott's students F. W. Warburton, N. F. H. Butcher, F. Akil, R. Bath and A. Z. Saleh, it looks as though the research which he directs has turned in that more promising direction.

Suppose that someone, with remarkable persistence, were to collect the records of 700 experiments on breathing, each curve starting at the word 'Go' and continuing for 900 sec. Would the grand-total breathing curve, formed by adding the ordinates of the separate curves, be likely to tell us anything about breathing? Probably very little, because the different rhythms would blur each other out. It would probably be far more instructive to study the breathing curves one at a time.

If that be accepted, another question follows. Would it be suitable to apply, to a single breathing curve, Schuster's method of periodogram analysis? I think not, because the periodogram is related to the ideal of a strictly periodic function $y$, which oscillates with a fixed amplitude according to

$$\frac{d^2y}{dt^2}+\omega^2 y=0, \tag{7}$$

where $\omega^2$ is a positive constant; whereas the period of an individual's breathing wanders irregularly, so that the salient facts would be blurred in the periodogram.

G. U. Yule (32) in 1927 had become discontented with simple harmonic motion. He took instead as his ideal the well-known equation for damped harmonic motion, namely,

$$\frac{d^2y}{dt^2}+2k\frac{dy}{dt}+\omega^2 y=0, \tag{8}$$

where $k$ and $\omega^2$ are positive constants. Yule developed a statistical method, intended to replace the Schuster periodogram, and to determine $k$ as well as $\omega^2$. Yule thus, I suggest, took several steps in the right direction, but did not go far enough. For in 1926 B. van der Pol (18) published an ideal, which he called a 'relaxation-oscillation', which is much more appropriate to mental waves than is damped harmonic motion. In a relaxation-oscillation most of the cycle is describable deterministically by a differential equation, but there may be a pause which comes to an end when a chance disturbance triggers-off the next motion. The record consists of a succession of similar pieces spaced at unequal intervals. Anson & Pearson (2) in 1922 had studied the flashing of a neon lamp when connected in parallel with a condenser, fed from a battery through a resistance. This is typically a relaxation-oscillation. B. van der Pol and J. van der Mark (19) made an electrical apparatus, comprising neon lamps, which imitated the heart-beat remarkably well. W. McDougall (10), in a paper 'On the seat of the psycho-physical processes', had argued in 1901 that the passage of the nerve current through a synapse is a discontinuous process comparable with sparking between the knobs of a Wimshurst machine, and that each spark generates a pulse of sensation. L. F. Richardson (22) in 1930 developed McDougall's idea further by arranging sixteen analogies between mental images and sparks. The most relevant kind of spark seemed to be that inside a neon lamp. Richardson therefore went on to investigate the behaviour of a neon, or more strictly an 'Osglim' lamp, and found

mathematical descriptions of its behaviour both in the Anson-Pearson circuit wher a single lamp flashes intermittently, and in Richardson's distraction circuit, where eithe of two lamps inhibits the other (23, 24).

B. van der Pol's fertile ideas were originally about oscillations described by

$$\frac{d^2y}{dt^2} - \epsilon\,(1-y^2)\,\frac{dy}{dt} + y = 0, \qquad (9$$

where $\epsilon$ is constant and $\epsilon \gg 1$ ((18), equations (6) and (7*a*)). He seems to have been unde the impression that this described oscillations involving the neon lamp. Richardson however, found by experiment on an Anson-Pearson circuit that its oscillations wer described by

$$\frac{d^2 \log y}{dt^2} + (\alpha + \beta y)\,\frac{d \log y}{dt} + y - 1 = 0, \qquad (10$$

where $\alpha$ and $\beta$ are positive constants ((23), pp. 319 and 329).

In the analysis of a single work-curve it would be advisable, as I suggested fourtee years ago (25), to think in terms of relaxational, rather than harmonic phenomena.

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# REPLY TO DR RICHARDSON'S CRITICISMS

By S. J. F. PHILPOTT, *University College, London*

I. *Introduction* (p. 177). II. *The two theories* (pp. 177–180). III. *Richardson's objections* (pp. 180–190): (i) *In general* (pp. 180–181); (ii) *Objections in detail* (pp. 181–190). IV. *Conclusions* (pp. 190–191). *Additional references* (pp. 191).

## I. Introduction

I was more than delighted when I knew that Dr Richardson had marshalled some of the objections to my theory of the curve. Controversy is one of the main processes in the advancement of science. It is impossible to anticipate all the possible objections that will be raised to a new theory or the difficulties that those reading the theory for the first time will find with it.

I propose to begin with a general picture, showing how I regard the whole, of which Dr Richardson is attacking certain parts or aspects. It need not take long. Much of what we need can be given by comparing Dr Richardson's general notions on the subject with my own. If I may, I will speak of 'his theory' and 'my theory'. A few of the main issues thus having been illustrated we can pass to a discussion of his objections in detail, any experimental or statistical points needed to make things clear being given when required.

## II. The two theories

The essentials of Dr Richardson's picture of the curve can be gleaned from his XIth section. He begins by saying (of my research) that he finds little of interest in grand-total curves, the more promising line of research seeming to him to lie in the study of single curves. Grand-total curves are again deprecated when the breathing curve with its slightly wandering period is taken as an illustration, and once more the need to study single cases is stressed. He ends by saying that when analyzing single curves it would be advisable to think in terms of relaxational rather than harmonic phenomena.

There is here outlined a possible programme of research as well as a tentative theory. Each step in it has obviously been checked on theoretical grounds. Thus, for example, if we grant that mental waves have slightly wandering periods it would follow that grand-total curves should have blurred outlines, and so be unlikely to afford evidence as to the nature of the underlying manifestations. Or again, even with single curves, periodogram analysis or kindred methods should *not* be used, for they are only suitable for curves in which more exact periods can be expected.

This cross-checking of possible moves is of the very essence of scientific inquiry. It also happens when we engage in controversy. Thus, for instance, some of Dr Richardson's comments on my work are much better understood if his own theoretical picture is borne in mind. His Objection I is inspired by the notion of slightly irregular periods. In other words, his own theory is peeping through. Sometimes he assumes *my* theory and its possible consequences as when he *approves* the use of periodogram analysis in Objection IV. He thus seems to contradict his own statement that such methods are inapplicable. The situation is made even more piquant when it is realized that most of my periodograms were on grand-total curves, i.e. the kind of curve he says should be so blurred as to be useless for purposes of analysis. If, however, we realize that for the moment he is

assuming my own notion of exact periods, the apparent anomaly is resolved. For by his interpretation of the consequences of my theory, the periodogram method ought to lead to significant results—provided I am right. And he therefore says so. As it happens by my own interpretation of the consequences of my own theory, I think his final conclusion (as to significance) is most unlikely. But that is another matter. Here we need only to note the way theoretical implications will be embedded in the criticisms, and, of course, in my replies.

To get my own theory in perspective it will be as well to begin with a few details of the work. It derived from two fields of inquiry; it has two sides or aspects. In practice much depended on cross-checking between the two fields (call them Part I and Part II) and on interaction between the two sides of the theoretical picture as it developed.

Work in the first field had to do with single experimental records or, at most, the curves obtained by adding together up to 40 or 50 records. It was in this field that early measures were made in terms of 'ratios'. If, for example, a curve had crests at or near the times $22\frac{1}{2}$, 45, 90, 180, 360, ... sec., it was said to contain a geometric 'wave' of ratio 2·00. And so for other possible ratios. This early stage in Part I of the work did not last long. (The years 1919–20 saw most of it.) It stopped quite suddenly when I noticed that most of the ratios already accumulated were related in that their logs advanced by steps of 0·0160 from 0·0000. The agreement was not exact, but it was close.

There was here a finding difficult to accept. *A priori* one would have expected the measures to be unrelated. They were from different curves produced by a number of different people working sometimes at different tasks. Why then a relationship at all? Why the apparent 'unit' 0·0160?

It was at this point that I decided to leave all measures exactly as they were. I did not revise them; I was chary even of making more. That they might have to be revised was obvious, but the time was not yet. At the same time, a theory began to develop. If the findings were sound then (i) the people concerned were behaving as though controlled by time-mechanisms of standard pattern, working to integral multiples of the given unit (or that unit as ultimately revised), while (ii) this unit of logarithmic periodicity did not seem to drift or wander because of such progressively changing tendencies as practice or fatigue.

A word is necessary on the measures themselves, especially as they are so heavily attacked by Dr Richardson. The work was confessedly exploratory, and I had been content to skim just one or perhaps two dominant 'ratios' from any one given curve. There was no attempt at exhaustive analysis analogous with that made (say) of the various harmonics in the note of a musical instrument. What kind of elaborate method I might have employed had the original study not terminated as abruptly as it did one cannot say. As it was, taking just the measures I had skimmed from the various curves, I found them falling into a system covering those curves *as a group*. The individual curve was no longer the main object of study. It was the *system* that mattered. Could enough be discovered about it for it to be defined?

Part II of the work began to develop just a little later than this. It quite quickly turned into a study of grand-total curves. The term is elastic. It means the sum (or average) of all the single curves of a given series of experiments, with $N = 100$ records as a bare minimum. Following up the consequences of certain hypotheses ((13), pp. 75–7; (14) p. 127) the question arose whether certain grand-total curves differed from one another

They proved to be very much alike, and quite apart from the specific hypotheses one was considering at the time there arose a general problem. As it was, the curves were too much alike, and there was again a situation that seemed *a priori* impossible.

A few words are necessary for those unacquainted with the work. The problem is to prove resemblances between grand-total curves. But that means accumulating thousands of records. My early results, based on just over 700 records, depended on evidence that *growth* of resemblance was observable as well as on actual resemblances between my four grand totals of weights between $N=100$ and $N=250$. More recent team work among my students seems, however, to put the matter beyond a peradventure. Briefly summarized, a study of the many correlations now on record shows that for curves of weights $N=1$ (i.e. single records), $N=100$ and $N=600$ the correlations are round about $r=0{\cdot}001$, $r=0{\cdot}300$ and $r=0{\cdot}600$ respectively. As will be realized, correlations of this kind must be explained. They cannot be swept on one side as accidental, especially when they are not isolated values, but things easily repeated. (For details see theses mentioned in list of references.)

Coming back to the main thread of our discussion: If John Jones and William Smith, working independently, produce their respective grand-total curves, and if they tend to rise and fall in parallel (the kind of resemblance claimed, for basal trend is eliminated before we correlate), then once more we are faced with two assumptions, namely, that (i) each of the persons concerned is behaving as though controlled by a standard timing mechanism and (ii) this mechanism is not subject to 'drift' or 'wandering' due to such causes as fatigue, practice, etc. In other words, work on grand-total curves brings us back by a very different route to the same two conclusions as work on single curves. Parts I and II of the research cross-checked. It is not that the results were similar in kind. On the one side, *different* curves from *different* people, etc., were related in that they seemed to share a standard unit $P=0{\cdot}0160$. On the other side there were no differences in this sense. One was measuring resemblances. Moreover, there was no explicit reference to a unit. Yet both lines of inquiry pointed in the same general direction. So that whatever the faults in method the results seemed to lead somewhere.

With these brief glimpses of work in the two fields we must turn to the problem of summarising the results in a way permitting us to arrive at a theory. In so doing I will use an approach outlined twice recently (14, 15). Consider, therefore, two basal findings and four deductions stated as follows:

*Findings:*

(*a*) Single curves differ widely in outline, whereas

(*b*) Grand-total curves resemble one another, i.e. they approach a standard patterning in their ups and downs.

*Deductions:*

(i) If resemblances between grand-total curves derive from the fact that each contains many or most of a standard set of waves of log periods $a\ b\ c\ d\ldots n$, then given waves must adhere so closely to their nominal periods that, for all practical purposes, we can claim rigid obedience. For if a given wave varies slightly from grand total to grand total it will reduce correlation between them.

(ii) Each given wave must have its own fixed series of trough times, for if any one given

wave varied randomly in phase from grand total to grand total it would reduce correlatio between them.

(iii) If under (i) we assume fixed periods, they will either have a highest commo factor or not. If they have, it can be used as a unit whether of period or of log time Call it $p$.

(iv) If under (ii) we assume the existence of a number of rigidly defined series of times they may (or may not) be related in that they would share a common member if produce one way or the other. If they do, let it be called $T_0$ and used as a zero point on the scal of log time.

Finding (*a*) derives from Part I of the work. As here expressed it could cover even th case of chance determined records. It is finding (*b*) from Part II that is of crucial importance *for if it is well founded the curves cannot possibly be randomly determined, and that hold whether for grand totals or single cases.*

Any adequate theory of the curve has to cover findings (*a*) and (*b*) together wit deductions (i) and (ii). Deductions (iii) and (iv) may also come in, but that is a separat issue. I myself claim that they do, and always include them. Finally, we must assum that the same theory and the same measures apply to all people, for there are no obviou individual differences.

The theory I have put forward ([13], pp. 75–7) suggests a pool of possible waves fro which, on any given occasion, the subject takes a random sample with which to determin his curve of the given experimental sitting. If a random sample is taken in this way a the beginning of every single experiment, finding (*a*) will follow, for to ensure a wid range of apparently different single curves we need merely suppose that the number waves per sample is small relative to the size of the pool. Finding (*b*) also follows, fo when enough single records have been obtained each and every wave in the pool will hav appeared and so will be present in the grand-total curve. Since this applies to all gran totals they must obviously tend to resemble one another. Incidentally, the theory give a broadly general definition of the standard patterning of ups and downs mentioned i finding (*b*). It is presumably that given when all the waves of the pool are simultaneousl excited.

This theory of random sampling from a pool of possible waves should be accompanie by an account of the search for the constants $p$ and $T_0$. Since, however, it is on thos constants that Dr Richardson's attacks are concentrated, it will be more convenient t deal with them later.

We can therefore close the present section by summarizing the general notions Dr Richardson and myself. He deprecates work on grand totals and suggests that perio are not really rigid. My own work, on the contrary, based both on single curves (Part and on grand totals (Part II), leads to the explicit conclusion that periods can be regarde as being exact, so much so that it would be difficult to explain the experimental findin if that assumption were not made.

## III. Richardson's objections

### (i) *In general*

Perhaps the quickest way for me to deal with his introductory sections (I–IV) will to summarize, annotating as we go to show where I disagree. His more formal objectio can then be taken in detail later.

His introductory sections refer to what he calls my startling claim to have *measured* $\cdot 076 \times 10^{-23}$ sec. Personally, of course, I should never use the term 'measured' in this onnexion. He then quotes early researches on such things as the size of molecules as being nalogous with the calculation of $T_0$, analogies that seem to me to be off the point. Noting that such measures are possible even with poor apparatus as long as there is trustworthy theory, Dr Richardson proceeds, by an obvious transition, to inquire just what my theory is. At this point I expected, of course, a statement in some such terms s those just given. But dear me, no. Apparently my theory is merely a *belief* in the xistence of waves of constant period. The statement is made that I sought collateral vidence in chemical reactions and in the log time of E. A. Milne (notions that did not nter in any way into my theorizing). Dr Richardson then sums up by asking just how ar one *ought to imagine that mental waves are strictly periodic.*

In all this there is absolutely no hint as to the real nature of my theory. Quite recently have reiterated the meaning of findings (*a*) and (*b*) and deductions to be made from hem (14,16). Dr Richardson makes no reference to that side of things. Instead, he tries to get he reader to accept his derogatory statement that my theory is just a belief, just one ossible *a priori* notion out of many. Then, with an air of sweet reasonableness, he suggests hat any sensible person ought to accept his notion of wandering periods as illustrated in elaxational oscillations.

All theories are beliefs. But one has to face facts with them. Where is the evidence for Dr Richardson's own notions? His argument is partly by analogy with such bodily hythms as breathing; partly by analogy with his experiments using Osglim lamps. *But rgument by analogy is not enough. In any case, what is Dr Richardson's reaction to the xplicit statement of fact that grand-total curves resemble one another? Nowhere in his present rticle does Dr Richardson even mention it. Why not?*

(ii) *Objections in detail*

It will be convenient to take them slightly out of order.

*Objection I* is simply answered. It asks whether we ought to imagine that mental aves have exact periods. To maintain an *a priori* objection of the kind would mean psetting finding (*b*) and deductions (i) and (ii).

*Objection III.* Having applied a test devised by M. G. Kendall to my grand-total curve (14), Fig. 1 A), Dr Richardson suggests that the curve 'may be mostly random'. There are any such tests. One could stop to ask whether this particular one is really applicable o a curve as heavily averaged as a grand total based on over 700 single records. But aiving that point we can still ask just what the test really tells us. Take an analogy. If human being is $1{\cdot}77\sigma$ below the average stature for males, that person *might* be mostly asculine, or, on the other hand, might be very completely feminine. In other words, we eed another criterion to help us decide between the two possibilities.

Before discussing other criteria, let Kendall's test be applied to a curve of known ructure, one we know of a certainty to be rigidly determined, every point in it having een calculated by a stated rule. It is the 'theoretical' curve given as Fig. 2 in my evious article (14). As there shown it has 133 ordinates with 94 reversals. The chance pectation is $2(133-2)/3 = 87{\cdot}33$ with $\sigma = 4{\cdot}83$. *In other words, the test fails to tell us that e curve is not chance determined.* Instead, it implies that it may well be 'mostly ndom'.

What now of another criterion? As Dr Richardson remarked when introducing th subject of my grand-total curve, I had quoted it as 'typical'. It is typical because i ups and downs move so freely. It is not damped in the amplitude of its movements a much as it might have been considering how many hundreds of records were summed t make it. I further noted that by analysis of variance it was quite easy to get highl significant figures in this matter of departure from the chance level of damping. When I went further to claim for such curves that

> their ups and downs are manifestations of underlying fluctuations of law-abiding nature...the peop concerned...must, as it were, have been working to...times of chronoscopic accuracy, now rising crests, now dropping into troughs, according to a master scheme or system binding on them all....(1 p. 125.)

Dr Richardson in his present attack does not mention these convincing results by analys of variance; he does not mention the deductions drawn from them. He ignores a criteric that shows that the ups and downs are far from the chance level in respect of dampin in their *amplitudes*. Instead, he claims that their complexity and *number*, being much a they would be in random zigzags, shows that they ought not to be regarded as anythin but 'mostly random'. The real interpretation is probably that a typical mental wor curve is so complex that it literally has as many ups and downs as a random zigzag. W need therefore not the Kendall test, but something more penetrating. The test of analys of variance is presumably more penetrating in this sense.

In other words, I would suggest that Objection III fails (*a*) because the Kendall te is not suitable for use with such complex and heavily averaged material, and (*b*) becau there already exists so much evidence from such methods as analysis of variance th grand-total curves are *not* likely to be chance determined, even though the Kendall te suggests that they *may* be.

*Objection IV*. It is not easy to deal with this objection in a few words. I am said to ha done things I did not do; it is suggested that I ought to have done things that, in m opinion, were not feasible. Take a simple example. Dr Richardson says:

> The interpretation of the periodogram requires a test of statistical significance. Such tests are availab ...But I cannot find any in Philpott's work. Nor are his periodograms published in such a manner th the reader can apply them for himself.

That sounds explicit and authoritative. Yet it is quite off the mark. When I publish ((13), p. 88) I specifically said that my data did not fall within the limits that would ma a conclusive result possible, and that I proposed to use a simple method suitable f exploratory work only. I know of no test of significance applicable to results by th simple method. Nor have I any idea of the data likely to be required for its applicati if ever it were devised. How then could I have been expected to publish, nineteen yea ago, in a way permitting the 'reader to apply the (as yet unborn) test for himself'?

At the time I chose not to apply the more formal method, and said why. Critics a entitled to attack me for so doing. But that is not the charge embodied in the quotati from Dr Richardson given above.

A slightly different form of the same trouble is found in the charge that (*a*) my origin measures and (*b*) my discussion of a recent curve ((14), Fig. 1B) show that I rega judging periods by eye as legitimate.

I will deal with problem (*a*) later. Here we may look at (*b*) to see what is wrong wi what I said about the given curve. For the criticism to be valid, I should, presumabl

have claimed that such and such a period was present. No such claim was made. The problem was very definitely of another kind, namely, to illustrate the fact that any long enough curve may seem to change its dominant period as we pass from one end to the other. That was the major problem. It was necessary to discuss it because I wanted to go from that to a discussion of the theoretical curve, which is zoned in the same way. As a third but minor point in this respect, I had three theoretically perfect trough sequences (times) that I wanted to use later for a discussion of the way $T_0$ is calculated. In fact, I had several irons in the fire, and the establishment of some one given period, whether 'by eye' or otherwise, was about the furthest thing from my mind at the time. Consider now a few extracts from the article:

> Fig. 1 B has been given to enable at least two of the sequences to be checked against an actual curve... dots under troughs in zone A are at times belonging to the first sequence... up to the 180th second there is a pretty close fit. After that... the sequence seems to die out....(14), p. 126.)

In analogous terms I showed that the second sequence fits, more or less, within zone B, but not elsewhere. Proceeding:

> In all probability, the manifestations are due to complex interference effects. The one thing we ought *not* to assume is that the underlying elementary waves are changing... it is merely the interference effects that fade in and out as we pass from zone to zone....(14), p. 126.)

Passing on to discuss the theoretical curve, and carrying the same problem into that discussion, I finally dismissed the matter. Since the depth of any given point in the theoretical curve is proportional to the number of divisors in the time value concerned, the argument ran:

> By definition, we know that deep troughs occur at highly factorizable time values. *But highly factorizable numbers are not equidistant on the scale of natural numbers*....(14), p. 132.)

Hence the obvious conclusion that no single set of equidistant dots could cover the complex manifestations under discussion.

It was a case of this kind that Dr Richardson cites as evidence that even in 1949 I regarded spotting periods by eye as legitimate. In fact, there was no claim to identify a given period. The whole discussion was essentially theoretical. Once more, therefore, I am bound to suggest that a criticism, couched in explicit and authoritative language and citing an actual example, is nevertheless quite off the mark when the example is put back into its context.

The method of judging by eye is not always discreditable. In the early days I found myself very definitely interpreting some of my correlograms in that way. Realizing the paradoxical situation, I began pedantically to work correlograms *of* correlograms. After a couple of these I decided after all to work 'by eye' and overlook the obvious humour of the fact that the discredited method was being employed when evaluating results by a reputable method. M. G. Kendall speaks of the same problem. Discussing three actual examples he says:

> ...the important point is not whether a particular serial coefficient is significant but whether the oscillatory character of the correlogram as a whole is so. *Here we have to form an intuitive judgment* but there is no doubt that the undulations...(in the three given cases)...are not accidental [my italics]. (9), pp. 412–13.)

'Forming an intuitive judgment' is a more emotionally satisfying term than 'using the discredited method of', etc. I would therefore beg to have it thought that if I have

sometimes seemed to put too many dots under curves it was done under circumstance where the nicer term could apply.

I am not a statistician, but it is obvious from the literature that the methods o periodogram or correlogram analysis are designed for use with cases where there are onl a few periods involved. When there are more, things become complicated. It will b remembered that Dr Richardson condemns the methods—provided his own theory holds I claim here the same liberty of opinion. As far as I can judge the curves are far to complex and far too short to give reliable results, and the latter fault cannot be remedie for the system is logarithmic. I said this when introducing the problem years ag ((13), p. 88); I still feel the same. Even in view of practical details there are difficulties The curves are by 'amounts per five seconds' or, more rarely, 'per second'. To get ther into a form suitable for the assessment of log periods they must be brought to 'mea rates per second per given log interval', a tricky business in itself, for to get the lo interval small enough a lot of interpolation is required. The ensuing calculations, whethe for periodograms or correlograms, are simple but infinitely tedious. Finally, after month of work, the results I obtained were much less instructive than those already afforded b much simpler techniques. I tried to make some use of the findings, as shown by m comments in the monograph (13), but, as pioneering tools, the two methods were simpl not worth while.

On the problem of finding significance in complicated cases we can close with anothe quotation from M. G. Kendall. Summing up his section on tests of significance h concludes:

> ... What does not follow is that if one intensity is significant then others must be so, even if they excee the significance values; for they are not independent of the significant value, at least for short serie What we ought to do, perhaps, is to extract the component which is considered significant from the seri and then analyze the remainder; and so on, as long as significant terms appear. This is hardly a practica computational possibility. Tests of significance in the periodogram, as in the correlogram, remai undiscovered. ((9), p. 434.)

In other words, and again admitting that I am not a statistician, I fail to see just ho (in our present state of knowledge) the methods in question could have been used i place of my more elementary routines.

I have saved until the end the charge against my original measures, the charge labelled (*a*) when we began this discussion:

> The strictly periodic waves which Dr Philpott claims to have observed were first noticed by a di credited method.... (p. 172.)

Once more, the only thing to do is to put the matter into its context and ask if the charg is really justified.

The key thought behind my original gropings was that troughs represent times o 'spells' of recuperation following 'spells' of active work. What more natural, therefor than to find mean rates of work over successive pairs of crests and troughs? The idea, o course, was that they should plot out a smooth curve. The work soon settled into a fou stage routine (exemplified in all four stages in twelve of the twenty figures of Part I o my monograph (13)). It ran (i) work-curve, (ii) spell-curve, (iii) whole-duration smoothin and (iv) part-duration smoothing.

The first two stages need not delay us. The work-curve was conventional; the 'spel curve', devised for a line of work dropped almost immediately, was kept in general u

or a time because it sometimes gave a hint as to a major rhythm. Our present interest egins with stage (iii). The argument behind it was mentioned a moment ago. Granted hint as to a major period, mean rates of output were calculated between successive airs of crests or successive pairs of troughs, or in some such way covering complete log eriods. Let us take Fig. 2 of the monograph (13) as an example. The spell-curve (Fig. 2A) ave an obvious hint that a ratio 2·00 wave is present with crests at $22\frac{1}{2}$, 45, 90, 180, . . ., sec. Iean rates of mental work were therefore calculated over the intervals between successive rests and plotted to give Fig. 2E, a curve that is completely smooth and even shapely.

This was the first check on the suggestion that a ratio 2·00 wave is present. As in all uch cases, the criterion was that the whole-duration curve had to be smooth. A cross-heck was made by slightly shifting the intervals and calculating mean rates as before. As seen in Fig. 2D the resulting curve is neither smooth nor shapely. Fig. 2E was therefore ccepted as the better. Incidentally, curves like Fig. 2D show how easy it is to depart rom smoothness in such cases and (more importantly) how difficult it might be to attain t were the data at all recalcitrant.

The fourth stage, namely, that of part-duration smoothing, was similar to the one just escribed save that complete durations were split into halves, thirds or quarters (rarely nto smaller fractions). In the present case, mean rates per second were calculated over hirds of durations, every third such interval being placed astride one of the crests. ymbolically, output rates were expected to run + − − + − − and so on, with mean ates of work over crests higher than those over trough areas. That this was actually so an be seen in Fig. 2C, where the expected rhythm is found save during the first 25 sec. f work.

Table 1

| | 6 | 11 | $22\frac{1}{2}$ | 45 | 90 | 180 | 360 | 720 | 1440 sec. |
|---|---|---|---|---|---|---|---|---|---|
| Fig. 2C | | | ? ? − − + − − + − − + − ? | | | | | | |
| 5E | | | ? ? ? − + − − + − − + − − + − − + − − + − ? | | | | | | |
| 9C | ? ? − + − − + − − + − − + − − + ? | | | | | | | | |

It was in this way that examples showing the ratio 2·00 pulsation were studied. There ere quite a number of them. Some gave a better rhythm on a half-duration basis. Of hose conforming to the one-third duration system three are shown in Table 1. The bottom ne is by intervals correct to the nearest second. For that reason sequence of crest and rough points can be traced closer to the origin. The other two curves are per 5 sec., one f them running for over 1000 sec. As will be seen, all three conform pretty closely ithin the lengths of curve available.

In experimental work of this kind there is room for error. Nevertheless, results such s those in Table 1 led to at least two generalizations, namely, that (i) if the same wave ccurs in two or more curves it tends to be constant in ratio or log period and (ii) its crest nd trough times tend to be the same from curve to curve.

In other words, one arrived at the notions expressed some years later in deductions ) and (ii) of p. 179 above. There is here, of course, the cross-check between Parts I and II the work that was stressed in our introductory section. It is mentioned once again at we may emphasize one of its implications, namely, that the results in Part I cannot ve been too faulty or erratic, otherwise regularities could not have appeared in them. o sum up, therefore: The charge that the original measures were made by a discredited

method, namely, 'guessing by eye', is not well founded; whatever the faults of the fou stage routine, it did at any rate produce results of the kind later predicted.

*Objections II and V.* These cover a series of attacks on $T_0$, a series long enough justify a separate article. Dr Richardson gives many alternative values for that constan It is therefore essential that we begin with an account of the reasoning behind my ow calculations. The matter has twice been summarized lately (14, 11). Dr Richardson quot the first paper. I will therefore assume the summary there given, elaborating on slightly to give a new approach.

Ignoring $T_0$ for the moment (it will turn up again as the time in seconds correspondi with zero on a scale of log time), consider how one could systematize a set of tin sequences. Suppose we have just two, namely,

$$\ldots \quad 10 \quad 15 \quad 20 \quad 25 \quad 30 \quad 35 \quad \ldots \text{ sec.}, \qquad (1)$$

$$\ldots \quad 12 \quad 18 \quad 24 \quad 30 \quad 36 \quad \ldots \text{ sec.}, \qquad (2)$$

it being assumed that both can be produced in either direction. Clearly, if we do produce them we shall find they have the following times in common:

$$0 \quad 30 \quad 60 \quad 90 \quad 120 \text{ sec.} \qquad (3)$$

The times in (3) can be interpreted in several ways. They have a period equal to t L.C.M. of the contributory periods 5 and 6. Or again, if the times in (1) and (2) show whe output is minimal in two given work-curves, those in (3) will indicate where troughs extra depth will occur if we add the curves symbolized in (1) and (2) together. It m help in understanding Table 3 when we come to it if we here note that each of the tim in (3) can be expressed as $30n$, where 30 is the L.C.M. of the contributory periods and takes the successive values, 0, 1, 2, 3, ... and so on. (In calculating $T_0$ we shall arrive a value $7920n$, which is strictly analogous with $30n$.) Finally, we may note that successi time values in (3) indicate salient points on a scale of time with its origin in zero. T notion of an analogous scale of log time, also with origin in zero, played a large part the reasoning that led up to the calculation of $T_0$. Hence the use of these prese illustrations. There were attendant problems that we have not mentioned, as, for examp that of expressing the system in log time units rather than in seconds of time. But the other problems are better ignored in a simple exposition, or left to unfold themselves the discussion proceeds.

Consider now a few assumptions as to a possible scale of log time suitable for use wi curves of mental work:

(i) Let the unit of log time be $p = 0{\cdot}0016$.

(ii) Let all mental waves have periods that are integral multiples of $p$.

(iii) Let the scale of log time be graduated in such fashion that the times at whi troughs occur for any given wave are integral multiples of its log period.

(iv) Let the scale be as short as possible.

(v) To ensure simplicity under (iii) and (iv) let empirical measures of log period trough timing be slightly modified where necessary.

To illustrate these assumptions it may be noted that the 'trough times' symboliz in sequences (1) and (2) are actually multiples of their respective periods. They w arranged that it should be so. Our present task is to describe attempts to reduce empirical trough sequences to a system of the same kind.

In the final attack on the problem I used just ten of the empirical waves—those I was most sure of. Their approximate periods and the time of at least one trough or minimum per wave are in Table 2. (There is no need to give more than one trough time per wave. The rest can be inferred.) For obvious reasons I chose times held in common by as many waves as possible. Thus, for example, the 900th second was chosen because no less than six of the ten waves have a trough near that point on the time line.

Table 2

| | Log periods | | | | | | | Trough times |
|---|---|---|---|---|---|---|---|---|
| (*a*) | 70 | 110 | 140 | 180 | 200 | 330 | | 900 sec. approx. |
| (*b*) | | | | 180 | | 300 | | 120–130 sec. approx. |
| (*c*) | 70 | 110 | 140 | 180 | 240 | 250 | 270 | 60– 70 sec. approx. |

The first task was to simplify or amend the figures as indicated in assumption (v). Wave $180p$ is the only one appearing in each of the three rows of the table. Tentatively, therefore, the trough times were amended on the assumption that their logs will be multiples of $180p$ apart. I thus arrived at times as follows.

| $63\frac{1}{2}$ | 123 | 900 sec. | |
|---|---|---|---|
| $(X-720)p$ | $(X-540)p$ | $Xp$ log time | (4) |

There are here two parallel scales of time. For one we have exact figures. We know how the values run as measured in seconds. On the other hand, we do not know how to express them on the log scale, save symbolically. The next task therefore was to solve for $X$.

Before indicating how a solution was found it should be noted that by fixing times as in (4) I found myself in difficulties with waves of log periods 70, 110 and 140. If they are to have troughs at $63\frac{1}{2}$ as well as at 900 sec. their periods should be divisible into 720. It was obvious that waves 70 and 140 could be brought into line, if need be, by making them 72 and 144. But wave 110 remained intractable until the final analysis had suggested a possible solution for it.

Coming back to the main problem, consider now Table 3 and the four rows of periods there suggested. They are for waves known to trough at 900 sec., and as will be seen, the

Table 3

| | Log periods | | | | | | L.C.M. | Zero point $=T_0$ |
|---|---|---|---|---|---|---|---|---|
| (*a*) | 70 | 110 | 140 | 180 | 200 | 330 | 138,600 | $1{\cdot}564 \times 10^{-219}$ sec. |
| (*b*) | 72 | 108 | 144 | 180 | 198 | 324 | 213,840 | $6{\cdot}460 \times 10^{-340}$ sec. |
| (*c*) | 72 | 112 | 144 | 180 | 198 | 336 | 55,440 | $1{\cdot}779 \times 10^{-86}$ sec. |
| (*d*) | 72 | 110 | 144 | 180 | 198 | 330 | 7,920 (15,840) | $4{\cdot}076 \times 10^{-23}$ sec. |

Four rows differ in respect of minor changes suggested in the periods concerned. In row (*a*) are the original or approximate periods given (as in Table 2) to the nearest ten log units. With them is their L.C.M. By assumption (iii) the log time $Xp$ should be divisible by the periods of all the waves concerned. It follows at once that $X$ must be equal to their L.C.M. or an integral multiple thereof. In other words, the solution suggested by row (*a*) is $X=138{,}600n$. Putting $n=1$, the simplest case, and calculating the time (in seconds) at which the zero point $0p$ would occur on the log scale, we get a possible value for $T_0$ as shown in the last column of the table.

As it happened, the solution was rejected. The L.C.M. is divisible by 300. By hypothesis, this would mean that wave $300p$ troughs at 900 sec. and it does not do so. Row (*b*) shows

the log periods amended to conform to nearby highly factorizable values. The solutio was rejected, for it would make wave 270 trough at 900 sec. Row (*c*) gives another possibl set of periods, and as before, the solution was rejected, this time because the L.C.M. divisible by 252 and the nearest empirical wave (approximate period $250p$) does no trough at 900 sec.

Row (*d*) gives the accepted result. Examining it to see why it was acceptable we not first that $X = 7920n$. Putting $n = 1$ we get log times as in (5(i)). There are no apparen anomalies in the log time set over against 900 sec. For the first time, therefore, we ca check by inquiring what happens at 123 seconds. The log time given there should b divisible by 300:

| | | | |
|---|---|---|---|
| $63\frac{1}{2}$ | 123 | 900 sec. | |
| 7,200 | 7,380 | 7,920 log time | (i) |
| 15,120 | 15,300 | 15,840 log time | (ii) |

It is not. We must therefore reject the times in (5(i)). The next step is to put $n = 2$. Th gives log times as in (5(ii)). The one at 123 sec. is now suitable. It is divisible by bot 300 and 180. We therefore make the last check by asking what happens at $63\frac{1}{2}$ sec. Th log time there given in (5(ii)) is divisible by all the periods said to trough at that point i Table 2 provided that for 250 we take 252 and either or both of the values 108 or 112 i place of 110.

The interplay between waves 108, 110 and 112 need not delay us. It is discusse elsewhere ((13), Table VI A). Here we can summarize at once by saying that all ten perio and all ten trough sequences (with others in the background) were thus brought into or system. It may be asked why I attempted to systematize in this way. It may be urge that the adjustments made in Table 3 amount to cooking the figures. And so on. F present purposes we cannot go into too many details. I chose to make the attempt systematization. The calculations must be judged in the light of what I was trying to d

It follows from what we have just said that the constants $p$ and $T_0$ must not be single out and treated as things in themselves. Their present meaning is wrapped up in th system from which they were derived. Together they define (*a*) that system and (*b*) a l time scale of quite interesting characteristics.

Advancing from the log time scale, we can find a use for the system. By assumption (i the log periods of waves that could conceivably be troughing at a given point on the l time scale are divisible into the time value concerned. It follows that *in an ideal gran total curve we are likely to find crests at times that are represented by prime numbers: we a likely to find deep troughs against times that are highly factorizable.* This is the notion behi my theoretical curve already mentioned a couple of times in these pages. I quote it aga because the very fact that it bears some resemblance to a typical grand-total curve evidence of the value of the constants $p$ and $T_0$. Any criticisms of them must be ma with that fact in mind.

With this rather long introductory statement of the method by which $T_0$ was calculat we may turn to Dr Richardson's detailed criticisms of that constant. They are so numero that it will be best to number them and answer each as briefly as may be.

(i) There are too many significant figures in $T_0$. Would I be content to jettison t ·076 provided I could persuade psychologists to accept the 4?

This did not arise directly under Dr Richardson's Objections II and V that we are here nswering, but it is best dealt with in association with them. The answer is that to use round figure would ruin the constant as a zero point on the time scale. There is no need or me to illustrate this, for the moment Dr Richardson begins to use $T_0$ himself he uses ll four significant figures and seven figure logs into the bargain (see his IX(3)).

(ii) $T_0$ was obtained by extrapolation and this is dangerous.

I agree if one is trying to measure, for example, the diameter of molecules. But that a bad analogy. More appropriate is the case of calculating forward to determine the ay of the week on which Christmas Day will fall in A.D. 2051. The necessary assumption n such a case is merely that the time scale remains rigid (i.e. that human governments re too busy to meddle with the present calendar). Once more, however, Dr Richardson omes to my help. Having attacked me in his § VI for extrapolating, he proceeds in his IX to calculate values for $T_0$ on his own account, going as far afield as $2{\cdot}389 \times 10^{-574}$ sec. Ie does so without even (*a*) a *pro forma* apology to the anti-extrapolationists or (*mirabile ictu*) without (*b*) asking if he may keep four significant figures in the answer. He is orse than I am.

(iii) 'He wished... to choose the numerical value of $T_0$ so that plenty of the ndividual waves of his series should have minima at $t = 900$ sec. By trial, he found that $T_0 = 4{\cdot}076 \times 10^{-23}$ sec. gave a remarkable crop of minima at $t = 900$ sec.'

I am reminded here of a Home Guard incident. The District Commander on a tour of nspection stood looking at a point being prepared for defence. After a long and almost ainful silence, he turned, scratched his head, and mildly inquired 'Must I be polite or an I say what I like?'

Put bluntly, I do not think any reasonable person could square this account of my aims nd procedure with what I myself said about Tables 2 and 3 a moment ago. Where are he waves 'in plenty' at $t = 900$ sec. in those tables? At what point in the inquiry did find, by trial, that the accepted value for $T_0$ would give a 'remarkable crop' of such inima? Whether in practice or in theory there are relatively few waves with troughs at 00 sec. Even in the theoretical curve, where one would expect an approach to the ideal, ne depth of the curve at that point is not outstanding ((14), Fig. 2).

(iv) 'When expressed in its prime factors $15{,}840 = 2^5 . 3^2 . 5 . 11$. Therefore $15{,}840/n$ is an nteger for $n = 1, 2, 3, 4, 5, 6, 8, 9, 11, 12$ and for many larger $n$. But (it) is not an integer or $n = 7, 13, 14....$

'The lowest exception, namely, that for $n = 7$ especially spoils the beauty of the scheme.'

Again, as with (iii), I hardly know what to say. Dr Richardson now has the bit between is teeth. He is taking the number 15,840 and doing things with it quite contrary to the pirit behind what I said when discussing the L.C.M. values of Table 3. *Why* should it spoil ne beauty of things if 15,840 is not divisible by 7? If it had been I should have rejected at sight. It would have spoiled the analysis (beauty or not) if it had been so.

(v) 'But we can easily remedy that.' Dr Richardson therefore proceeds to bring the mber 7 into the calculation. This gives a new value for $T_0$. Dr Richardson remarks t is easy to discover marvellous figures by such methods!'

I agree, but I am not the person doing it. Dr Richardson goes on to calculate quite number of variants, each more marvellous than the one before. But why do it? What gained? *In any case, will they fit the data?*

Bringing the matter down to bed-rock, let us take $T_0$ as it was amended in (v), i.e. b bringing in the factor 7, and inquiring whether it would work. The pragmatic test is alway best. It would give log times as in (6) below. Comparing with what was said of (5) it wi be remembered that numbers in the three positions have to be divisible by given lo

| 110,160$p$ | 110,340$p$ | 110,880$p$ | |
|---|---|---|---|
| 63½ | 123 | 900 sec. | (6 |

periods. Even a casual glance at Table 2 will show that the log time on the left ought t be divisible by 252 and it is not. That in the middle should be divisible by 300 and it i not. The one in the right ought *not* to be divisible by 252 but it is. There are other trouble We have said enough, however, to show that by bringing in the factor 7 to improve wha he calls the beauty of things Dr Richardson has spoiled their usefulness. I may hav been pedantic in trying to relate the empirical measures in this way, but I must be judge by that aim. Dr Richardson has invented a new aim and I repudiate it. Straw horses ar easily pushed over.

(vi) 'Let us suppose that it would suffice if the first $n$ of the elementary waves... ha minima at $t=900$ sec. and let us determine $T_0$ for successively increasing $n$....'

Here I am afraid Dr Richardson and I part company completely. The notion that I wan the log time set over against 900 sec. to be divisible by a straight run of the first $n$ natura numbers is a travesty. In any case, why should anyone want it? Of what strange an mysterious value would it be to us if we could ensure it?

(vii) 'There are many different numerical values of $T_0$ that seem reasonably likely t give a minimum at $t=900$ sec. It may be said that this objection is unfair becaus Dr Philpott was considering not merely a single trough but several sequences of trough and because he has published a comprehensive discussion of sequences in Appendix B o his monograph. (I do not understand that Appendix.)'

This is the only time Dr Richardson mentions the fact that my calculations involve several sequences of troughs. In his attacks he has been considering the trough a 900 sec. as a thing in itself—just, of course, as he has considered $T_0$ as a thing in itsel He has divorced both from their true context, namely, the *system* underlying th measures.

What Appendix B of the monograph has to do with the problem is not clear. I shou not call it a comprehensive discussion myself. If it were, an appendix would be a funn place to put it. As far as I am concerned the discussion was in the text of the monograp

## IV. Conclusions

The difficulty with my own theorizing is that I am dealing with a complex of observatio some of them seeming to run against common sense. Why, in Part I of the work, shou I have found measures falling into a series true for all people? Why, in Part II, maki quite different measures, should I have found all grand-total curves tending to be simil no matter by whom produced? Whatever it is I am trying to unearth is something th lies on a very different plane from such things as the breathing rhythm. This fact has be clearly understood before one can begin to appreciate either the difficulties of the wo or why people are so reluctant to face its consequences.

If I am right we are at the beginning of a new line of inquiry that will throw light o quite a number of things beside the simple manifestations of fluctuation of attentio

By correlating work-curves together and extracting factors from the matrix it is already clear from recent work by my students that there is a general factor, with a possible (weak) bipolar factor in the background. People differ in their general factor saturations. What does that mean? Granted that all must draw samples from the hypothetical pool of waves when beginning their experiments, so that they all use the same waves and approximate to the same ideal grand total, it would seem that there are individual differences in the *rate* at which they so approximate, those taking larger samples getting there more quickly. In other words, they get higher saturations with the general factor. Just what kind of people are they? Why do they draw bigger samples? Many questions arise. There is no limit to the amount of work that can be undertaken.

To turn to Dr Richardson's criticisms, his concluding remarks are largely to do with the attack on $T_0$, saying that in his opinion there is little one can do to save either the theory or the arithmetical measure I have suggested. Commenting on the titles of recent theses by my students (1, 21, 26, 30) he says (quite wrongly) that it looks as though we are moving away from what is to him the unprofitable study of grand-total curves towards a study of single curves. He concludes by re-emphasizing his belief in the analogy with the breathing curve, in wandering periods and the like, saying in his peroration that as he said fourteen years ago it would be better to think in terms of relaxational phenomena. Dr Richardson thus makes his own position clear. Unfortunately, it does not sound as though he has any evidence whatsoever. I would admit that, *a priori*, his theory seems comfortably orthodox and free from trouble. On the other hand, one must face facts. I do not know of anything in the field of mental fluctuations that would support his notions. The evidence from analysis of variance is dead against one of his main props, namely, the notion that if periods wander grand-total curves will become blurred.

Looking at the details of the theoretical picture I myself have tried to paint, I am quite open to conviction that this or that item may need to be changed. Sooner or later, someone will manage to improve on the values I have suggested for $p$ and $T_0$. Not that I think the time is yet ripe. The field of work is still split into Part I and Part II, into measures of period and trough sequences derived from weak or single curves on the one hand and into correlations between strong or grand-total curves on the other. Much remains to be done in the second field before we need return to the first. With enough workers they could of course be run side by side. But I still have a feeling we may find something we need on the grand-total side before going back.

## V. Additional references


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## COMMENT ON DR PHILPOTT'S REPLY

By LEWIS F. RICHARDSON

I have read Dr Philpott's reply and have considered whether I ought to modify any o my criticisms.

The same test for randomness ((9), p. 124, art. 21·43) has been applied to each of tw curves, alias polygons, in Dr Philpott's 1949 paper (14). One of them (Fig. 1 A) is a grand total work-curve; in the other (Fig. 2) the ordinate, reckoned downwards, represents th 'total number of factors in ten consecutive natural numbers'. The results are rathe similar. Both curves have rather more turning points, alias spikes, than one woul expect them to have, if they had been produced by purely random processes. The devia tions from expectation are not extreme, being respectively 1·77 and about 1·4 of th standard deviation, as given by Kendall's formulae. That is to say, by this test, bot curves could credibly be regarded as random. Dr Philpott rejects the test because it ha failed to tell us that the second curve was not chance determined; for in fact every poin of it was calculated by a stated rule from the natural numbers. He has thus raised a mos interesting question concerning the philosophy of chance and randomness. Hen Poincaré (17), in his book *Science and Method*, asked how the concept of chance can aris in a world that he regarded as deterministic. One of Poincaré's answers (p. 75) is tha 'we attribute them to chance because their causes are too complicated and too numerous The causes of the aforesaid Fig. 2 were moderately numerous and complicated, namely a long run of the natural numbers, counting how many factors less than 361 each ha and totalling over ten of the original numbers. I suggest: (i) that these causes may we have been sufficient to make Fig. 2 random in Poincaré's sense, (ii) so that it is unde standable why Kendall's turning-point test has given us a warning about it, (iii) that there fore Dr Philpott's objection to my application of the same test to his Fig. 1 A is washe out.

The curve derived from the natural numbers ((14), Fig. 2) is offered by Dr Philpott a a theoretical curve of fluctuation; and, strange to say, it can hardly be analyzed. Fo in its construction, he took totals over ten consecutive elements, and such totalling is a irreversible operation. Even when we know the rule by which the curve was constructe we cannot, from its ordinates, deduce the natural numbers. One of the obstacles t understanding Dr Philpott may be that, where most research workers would attempt a analysis, he is content with a synthesis. However that may be, the following discussio via Bayes's theorem, applies to analysis and synthesis indiscriminately.

As a philosophical by-product of this controversy we are left with the question whethe irreversibility of calculation can ever be an ingredient of the concepts of chance an randomness? I do not know.

The general pattern of this controversy is illuminated by Prof. H. Jeffreys's theory probability. Jeffreys (6) regarded probability as a numerical expression of a degree reasonable belief, 0 denoting certainty that the proposition in question is false, and denoting certainty that it is true.

There is a theorem, derived from Bayes, whereby Jeffreys compares the probabilities rival hypotheses. It may be stated thus:

(the probability of each hypothesis after the experiments are known) varies as (the probability of the same hypothesis before the experiments are known) multiplied by (the probability of the experimental results if that hypothesis were true).

In all three brackets the same general information is supposed to be available in the background.

The three phrases in brackets are too long for repetitive use. Jeffreys states the theorem briefly thus (p. 46):

Posterior probability $\propto$ prior probability $\times$ likelihood, in which each short expression is a technical term defined by the *respective* longer phrase above. Let our rival hypotheses be: (i) the theory that the observed effects in grand-total curves are merely random, (ii) Dr Philpott's wave-theory. The degrees of belief which I shall assign are merely personal guesses, yet I think that they will illustrate, although perhaps in caricature, the perplexities which many people have experienced.

To estimate the prior probabilities we need to find people who are reasonable and learned, except that they never heard of Dr Philpott's experiments. There were plenty of such in the year 1920. We ask them in imagination two questions. Is it credible that the average of 700 work-curves would be merely random? Answer: Not improbable, except for initial and final spurts; probability say 0·5. Is it credible that the average of 700 work-curves would be explicable in terms of a time of $4{\cdot}076 \times 10^{-23}$ sec.? Answer: Quite incredible; probability zero.

To estimate the 'likelihood', remembering that it is a technical term defined as above, we have to put two questions to persons who have read Dr Philpott's publications. I take myself as a specimen of such. Is it credible that the fluctuations shown in the grand-total work-curve ((14), Fig. 1A) are merely random? Answer: Yes, by one of Kendall's tests, they may be. I reckon the likelihood, rather diffidently, as 0·08. That anyway will serve us a token value in the sequel. Is it credible that Dr Philpott's wave-theory can explain the fluctuations in the same curve? Answer: Yes, he certainly does explain some of the larger fluctuations. A lot of minor fluctuations he seems to leave undiscussed. He makes a number of small adjustments, and deprecates the tendency to call them cookery. I do not know what the overall likelihood is, but for the sake of argument, I give him the benefit of various doubts, and put it in at the token value of 0·7.

Next the above degrees of belief are to be inserted into Bayes's theorem, so as to give the posterior probabilities, which purport to be degrees of reasonable belief in the rival hypotheses, when all the evidence is considered together:

$$\text{Posterior probability of the random hypothesis} \quad = 0{\cdot}5 \times 0{\cdot}08 = 0{\cdot}04,$$
$$\text{Posterior probability of Dr Philpott's wave-theory} = \quad 0 \times 0{\cdot}7 \quad = 0.$$

Thus the random hypothesis wins, because Dr Philpott contends against a prior probability of zero. Those who seek to prove the existence of precognition by experiments on card-guessing are in a similar difficulty. Let us consider what moves Dr Philpott could make in order to prevail over the random hypothesis. It would be of little avail for him to show more conclusively that his wave-theory explains the observed facts; for that likelihood is already guessed at 0·7, and it cannot rise above 1·0. He complains that I have ignored his analysis of variance ((14), p. 124). All he says about it there is confined to three lines. It would be more likely to receive attention if a systematic account were published in one of the statistical journals. But, anyway, what role could this analysis

of variance play in Bayes's theorem? It might, conceivably, reduce the 'likelihood' fo the random hypothesis from the 0·08, at which I have tentatively set it, to some numbe much nearer to zero. Even if the likelihood were actually zero for the random hypothesis that would not be a victory for Dr Philpott, but only a drawn game. He cannot win unles he can somehow remove the prior probability of his wave-theory a little bit away fron zero. Such is the strength of prejudice!

Another by-product of controversy here presents itself. Granted that Bayes's theoren gives an approximate account of the way in which people arrive at beliefs, is not multiplica tion by zero too strong an expression of prejudice? For how did such great innovators a Copernicus, Darwin and Einstein ever come to be believed?

Dr Philpott asks me emphatically why I have never alluded to his statement tha grand-total curves resemble one another. The answer is that I have never seen the proo of it set out with proper statistical clarity, such as one would expect to find in a statistica journal.

I must accept Dr Philpott's assertion, although without his emphasis, that my § IX headed 'The method of extrapolation' misrepresents the argument by which he calculate $T_0$. He rejects my § IX with vehemence; on the contrary, I now admit plainly what I ha previously indicated, namely that it is not *quite* fair, it is 'variations on a theme b Philpott', it presents clearly ideas that would be certain to arise if his theory were eve to become generally accepted, and it has performed the useful service of provoking hir to explain himself.

As to my other comments on his work, Dr Philpott's reply does not move me t withdraw or to modify any of them.

There are numerous minor disagreements not worthy of detailed comment. I should b content now for the arguments to 'go to the jury', which consists of readers of thi journal.

### Additional references

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# REPLY TO DR RICHARDSON'S FURTHER COMMENTS

By S. J. F. PHILPOTT

Dr Richardson has raised many points of interest in his further comments. To deal with them adequately would require another article. Here I cannot do more than reply to three problems, namely: (*a*) the comparison of results from Kendall's test and analysis of variance, (*b*) whether the evidence for resemblance between grand-total curves has been set out in statistical form and (*c*) whether my theory is adequately stated when Dr Richardson brings Bayes's theorem into account.

## The Kendall test: analysis of variance

Dr Richardson speaks throughout as though the Kendall test has universal validity. In my reply to his first comments I noted that it fails to pick out my theoretical curve as something *not* randomly determined. In his further comments Dr Richardson explains this by saying the theoretical curve is probably complex enough to behave as though it were random in the Poincaré sense. He adds that this probably washes out my objection to the test as applied to grand-total work-curves. In my own discussion of the matter I said that grand-total curves seem complex enough to have as many peaks and troughs as a chance-determined curve. So far, Dr Richardson and I are agreed. But we come to conflicting conclusions. My earlier illustration was in terms of overlapping distributions, showing that because of overlap we cannot efficiently distinguish the sex of given individuals in terms of measures of stature. We need a better criterion. So here, if the Kendall test is going to give trouble of the same kind, the right answer is not to use it. As Kendall (8) remarks when discussing the matter, there are many other tests of the kind, so much so that he lightly says the field is a 'happy hunting ground' for test-makers. In our own laboratory J. S. Small (27) has recently used three of those other tests, namely: (*a*) Kermack and Kendrick's run condition (1937), (*b*) Gleissberg's criterion for cyclic variation (1945) and (*c*) Gleissberg's sign-sequence condition (1947). In each case he found significant departure from the chance level, whether with my theoretical curve or with his own grand-total curve. In all three sets of results he thus runs counter to those obtained with the Kendall test. It sounds like a field where by carefully choosing the test one can prove anything. Or, alternatively, as a field where one needs to state specific problems to a statistician in order to get advice as to the safest test in the circumstances.

Much depends on the things we desire brought into account. In my own theorizing I have not been interested in the *number* of peaks and troughs in a single curve. The Kendall test therefore leaves me cold. I am much more interested in the *times* at which those peaks and crests occur *and whether those times are the same from curve to curve*, in other words, whether curves are correlated in this respect.

It is here that analysis of variance comes into the picture. Suppose we have 1000 experimental records, each extending over 100 time intervals. Let the data be set out in a table of 100 columns and 1000 rows where each row shows how output rose and fell in a given experiment, figures being expressed as deviations from the basal trend of the experimental record concerned. Let peaks and troughs in the first row be randomly determined, but (by some miracle) let it be supposed that each of the remaining rows is

an exact copy of the first. In other words, let correlation between rows be perfect i respect of crest and trough timings, and that despite the fact that (within any given row they are randomly placed. Let the figures in each column be summed and divided b $N = 1000$ to give the ordinates of the average (or grand-total) curve.

We have supposed that correlation between rows is perfect. It follows that each colum will contain a given value one thousand times repeated. Some columns will contain peal and others trough values. Or again, the grand-total curve will have the same outline a each of the contributory curves; its ups and downs will move as freely as theirs i amplitude. Of such relationships the Kendall test takes no account. Applied to th grand-total curve it would merely return it as having enough reversals to be chanc determined. By analysis of variance, however, we can show that the grand-total curv is far from being as blurred or damped as it would be were the curves uncorrelated. B studying damping we thus bring into account the correlation between contributory curves

In passing we must note that the hypothetical case considered above assumes (*a*) perfec correlation between contributory curves and (*b*) a random patterning of ups and down within any one curve. In practice, the correlation is very small. Nevertheless, if, becaus of *a priori* reasoning, we want to assume that each contributory curve has randoml distributed ups and downs, we shall later have to explain how a thousand or so differen random series of the kind could (*a*) be correlated even slightly from one to another an (*b*) how they could add together to produce a standard end result, namely, a grand-tota curve that correlates with other such curves. It could be explained if we postulat elements that are (as it were) standardized zigzags. It is much simpler to think in term of a pool of standardized waves, i.e. waves rigidly determined in periods and troug sequences.

Still, in passing, I would note a point made by Dr Richardson when he said that difficulty in understanding my notions is that I synthesize when others would be conten to analyze. One cannot synthesize in this matter without first analyzing. I would agre however, so far as to admit that I have probably not kept them clearly enough apar I should imagine, however, that the trouble sensed by Dr Richardson is not so much c confusion between analysis and synthesis as of relative emphasis on the relation *betwee* curves as distinct from what is happening *within* any given curve. I am interested i both, but my approach is usually by the first whereas (being wise after the even I realize that my critics have usually chosen the second.

### Evidence for resemblance between grand-total curves

Dr Richardson says the evidence here has not been set out in proper statistical form. T begin with, we had better note that the problem is of growth of resemblance with weig of material, correlation becoming well marked when curves are at grand-total level. Th evidence is mainly of that growth. I agree it is scattered and urgently needs summarizin But it is all on record and in full detail. Dr Richardson himself mentions the theses Warburton, Saleh, Akil and Rath. Each of these workers deals fully with the proble illustrating with his own data. Each gives results obtained by correlating curves; the la three used analysis of variance as well.

Before speaking further of present-day work let us look back to earlier days. My fir printed communication(12) was a short abstract written around the explicit statemen that curves from single experiments differ widely in outline, whereas grand-total curv

esemble one another. So that even as early as this the fundamental findings were clearly nvisaged. They are not of recent growth. The constants $p$ and $T_0$, on the other hand, ere not mentioned in that abstract, for it was written before $T_0$ was calculated. (Those nterested in the evolution of theories may like to turn up the reference to see how I was hinking before matters finally crystallized.)

At the time I was intrigued, and puzzled, by the presence of a *few* waves seeming to be arithmetic' in period as distinct from the *many* of 'geometric' (logarithmic) nature. The pparent arithmetic waves had periods

> ..between 55 and 60 seconds or simple fractions thereof (approximately 30, 20 or 15 seconds).... (12) p. 384.)

As these waves[1] seemed characteristic of grand-total curves, I gave a tentative theory of he grand total curve as follows:

> The evidence strongly suggests that pooling results in an averaging of the geometric elements with onsequent clarification of the arithmetic element or elements. ((12), p. 384.)

n my monograph of 1932 I dropped the notion of 'arithmetic' periods. With them went vhat may be called the 1929 theory. It was replaced by the 1932 theory of random ampling from a pool of general purposes waves of fixed logarithmic periods. Another hange in the 1932 presentation was that the evidence for resemblance between curves essential when discussing the theory) was now in terms of what I called 'shapes'. As it appens, that evidence was not as conclusive as I thought at the time. With the help of he students mentioned by Dr Richardson enough 'shapes' were later collected to make he results of this line of work statistically significant. They are discussed in my 1949 rticle.

Recent work, using the methods of analysis of variance and correlation or both, will be ound set out in detail in the theses already mentioned or in others (4,7) listed among the urther references at the end of this set of notes. Some of Saleh's correlations were quoted s illustrations in my earlier paper ((14), p. 137). A few further examples will perhaps be ustified. Thus, for example, Miss Suhasi Ghosh (5) had 60 men and 60 women subjects naking one attempt each at each of 10 tasks. This gave 1200 records all told. She pooled hem by tasks to get 10 curves of weight $N = 100$ records apiece. Applying analysis of ariance to the resulting $10 \times 110$ table she got significance with $p < 0{\cdot}01$. (That is to say, 0 curves each of grand-total weight resembled one another enough to cause the variance etween columns to depart significantly from the chance level.) Attacking the problem f resemblance from another angle, she correlated the 10 curves to get coefficients ranging rom $+0{\cdot}86$ down to $+0{\cdot}12$ with an average of $0{\cdot}32$ ($\sigma_r = 0{\cdot}10$). Rearranging the records y sex, she got two quite heavy grand-total curves of $N = 600$ each. The correlation etween them was 0·58.

[1] As noted in the 1929 abstract, the notion of 'arithmetic' periods came mainly from certain correlograms. n practice, I calculated correlograms and periodograms using (*a*) ordinary time intervals (usually 5 sec.) and ) logarithmic intervals. It was from the first-named set of correlograms that I seemed to get the information at all my grand-total curves were agreeing in having one of the arithmetic cycles as a best-fitting measure. ne correlograms in question are among those in Fig. 29 of my monograph (13). They are the ones I tended to terpret 'by eye' (see p. 183 above). Although correctly described in the accompanying text they are wrongly lled periodograms in the legend under the figure.

The main reason for dropping the notion of arithmetic periods was that they seemed to have troughs inciding with the sequence for log wave $180p$, troughs of which are at $63\frac{1}{2}$, 123, 239, 464, and 900 sec. It is teresting that this same sequence was invoked when calculating $T_0$.

Miss Eva Denton (3) obtained a curve of weight $N = 430$ from a group of women students By analysis of variance she got significance with $p < 0{\cdot}001$. Taking the grand-total curve of Akil and Rath (each of $N = 160$) and cutting their length to that of her own experiments she correlated to get $r_{ar} = 0{\cdot}48$, $r_{ad} = 0{\cdot}47$ and $r_{rd} = 0{\cdot}32$ ($\sigma_r = 0{\cdot}14$). Since the curves o Akil and Rath were based on their own personal output, the results show how the grand total curves from two individuals compare; they show how those individual men compar in their crest and trough timings with the average result of a group of women. Othe figures could be quoted. They are on record, but the time lag in printing being what it is it may be some time before they get wider circulation.

## Statement of the theory: Bayes's theorem

The first point I would make is that the theory should be separated from the figures wit which it may happen to have been associated at any given stage in its development. Th 1929 statement was faulty in that it mentioned 'arithmetic' and 'geometric' element It would have been better to give it quite generally, saying, for example, that resemblance between grand-total curves can be explained if in every single curve there are (*a*) a fe 'general' waves, i.e. waves that always appear and (*b*) a number of waves seeming t come as though by random sampling from a pool of 'general purposes' utility. Had i been given in some such form I might not have seemed to scrap it entirely when the notio of 'arithmetic' cycles was dropped, for as so expressed it only differs from the 1932 theor in postulating a few general elements.

The 1932 theory was stated generally in terms of random sampling from a pool of wave of geometric (logarithmic) nature. Nevertheless, I was at pains to express those waves i terms of the constants $p$ and $T_0$, and it is those constants that attract attentio Dr Richardson, for example, never speaks of the theory generally. He always speaks a though $T_0$ were the essential notion involved. Yet, as we have seen, I have given th theory twice over, clothing it in two seemingly different sets of figures. It would still b the same theory if someone managed to find yet a third way of expressing the empiric crest and trough times.

I have raised the problem of Bayes's theorem because of the way Dr Richardson frame the two hypotheses, those of chance and of my own idea of things respectively. To chanc he gives a prior probability of 0·5. This can be held to mean that of all the ways in whic Nature could conceivably engineer the structure of a grand-total work-curve, no less tha 50 % are so complex that we are constrained to say they are chance determined. He stat my hypothesis in very different terms. It does not cover a host of possible cases. On th contrary, he ties me down to one possibility asking:

> Is it credible that the average of 700 work-curves would be explicable in terms of a time of $4{\cdot}076 \times 10^-$ seconds? Answer: Quite incredible; probability zero.

The two hypotheses are not comparable. The scales are too heavily weighted in favo of the chance hypothesis, for, as we have said, it is permitted to cover many possible case

Consider therefore the effect of stating my own notions in the most general tern possible. I should imagine that the simple statement 'Work-curves tend to be correlate would meet the case to begin with, for from it we can deduce that grand-total curv should resemble one another, and so on. If we accept that as the hypothesis to be teste an appropriate null hypothesis would be that work-curves are *not* correlated. Since the

re only the two possibilities, those of correlation or no correlation, and if we take the hance probability to be 0·5 the same value must be given to my own hypothesis. We hus escape the zero probability allotted to it by Dr Richardson. We shall also have proeeded in the spirit of Bayes's postulate (not the same thing as his theorem), that in the bsence of contrary evidence the two prior probabilities should be taken as equal.

Incidentally, there arises here Dr Richardson's question as to how results from analysis f variance could be brought into account, instead of those from the Kendall test, when alculating the 'likelihood' of the chance hypothesis. The answer is that if we state the ypothesis in terms of correlation between curves the Kendall measure becomes inppropriate for it takes no account of correlation. By analysis of variance, the departure rom chance can be taken as $<0{\cdot}01$. Making these changes in Dr Richardson's figures, ve finished up with

$$\text{Posterior probability, random hypothesis} = 0{\cdot}5 \times 0{\cdot}01 = 0{\cdot}005,$$
$$\text{Posterior probability, Philpott hypothesis} = 0{\cdot}5 \times 0{\cdot}7 = 0{\cdot}35.$$

he exact figures mean little. The upshot is that my theory gains over that of chance in espect of correlation between curves. It must be noted, however, that we already know his from the usual tests of significance with $F$-ratios and so on. We have not gained much y using Bayes's theorem. (I should imagine the theorem is only valuable, if at all, when omparing specifically framed hypotheses of nearby meaning, but that is as far as a nontatistician like myself dare go in expressing intuitive objections.)

In closing, I must express my grateful thanks to Dr Richardson for marshalling some f the objections to my notions. I have known him for many years. I am never quite ure when he is acting as counsel for the prosecution, trying to get a conviction and an xemplary sentence, and when he is acting as devil's advocate before an approaching anonization. To illustrate the point I would note the delightful inconsistency shown hen he gives my notions a 'before experimentation' probability of zero, thus implying hey are too incredibly far-fetched ever to be believed, followed, a few sentences later, ith the very generous 'after experimentation' likelihood of 0·7. But whether in the one apacity or the other, one can depend on him to put up a good case.

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# BEHAVIOUR IN A CATASTROPHIC SITUATION: A PSYCHOLOGICAL STUDY OF REPORTS AND RUMOURS

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## I. Introduction

In June 1950, Darjeeling, a beautiful 7000 ft. high hill-station in the Himalayas, wit a population of 33,634 (1951 census provisional figure), suffered one of the worst disaster in its history. There had been landslides before, but nothing like this had ever happened Loss of life and damage to property were heavy and extensive. Hundreds of breache occurred on the Darjeeling-Siliguri road, the only motorable approach from the plains In the town itself houses collapsed and victims lay buried under the debris. For over tw days, rain poured incessantly down the hill slopes which continued to loosen and slide Collapsed houses and landslides lay across the roads, and mud flowed everywhere. Th water supply to the town was breached. The supply of electricity failed.

Over a hundred and fifty persons lost their lives in the district, about thirty of them i the town itself. Over a hundred were injured. More than two hundred houses wer damaged, and over 2000 people were rendered homeless. Refugees poured into the tow from the outlying districts with tales of woe and misery.

Rainfall alone had been the cause of the calamity. In three days no less than 32 i of rain fell, i.e. about one-third of the average annual rainfall. The daily figures (recorde at 8.30 a.m. I.S.T.) for the three critical days were:

| | |
|---|---|
| 11 June | 4·17 in. |
| 12 June | 17·88 in. |
| 13 June | 10·16 in. |
| Total | 32·21 in. |

Widespread anxiety was inevitable, but it remained within reasonable bounds. There w a feeling of instability and uncertainty similar to that which followed the Great India Earthquake of 1934 (Prasad (8)). Such situations of stress and strain produce emotion instability which affects the cognitive processes of the individual. The present investig tion was mainly directed towards studying the behaviour of individuals in such a insecure situation.

Most of the data for the study were collected during the disaster when it was still progress. Such phenomena are rare and can hardly be predicted. Naturally, no researc tools could have been prepared in advance. Moreover, for obvious reasons, a systemat and extensive study was impossible. The materials collected, however, have been pr sented here because they throw some light on the processes of perception and recall, a on the spread of rumours in a catastrophic situation.

## II. METHOD EMPLOYED IN COLLECTING DATA

arjeeling has about 3000 visitors in June. Reports and rumours were collected mostly om them. Coming from the plains and with scant experience of the hills, they were far om accurate in their appraisal of the data. There was a tendency to exaggerate the uation. Moreover, the second week of June being generally the time when the wnward exodus begins, they now had a more than usually urgent desire to go back. aturally, personal involvement in the crisis was great.

As far as possible, rumours were collected soon after they were heard. An attempt to rify them was made later and the amount of truth they contained was determined. me reports appearing in newspapers have also been analysed and recorded. Moreover, attempt was made to study by experimental methods both transformations in recall d distortions in perception.

## III. MATERIALS COLLECTED

### (i) *Reports*

eports collected centred mainly on (*a*) the damage to houses and human casualty, ) the amount of rainfall in three days, (*c*) the occurrence of earthquake, (*d*) the restora- on of water supply and electricity, and the condition of the road and railway to and om the plain, and (*e*) the idea of sin and God's will. The investigator tried to ascertain e amount of truth in each factual report either by going personally to the spot, or by aking careful inquiries at the scene of the occurrence.

*eports about damages and human casualty*

There were several reports about individual incidents and of these the reported de- uction of a Mr X's house, of Bhotia[1] *busti*, a village about a mile and a half from the wn, the family of Mr M, and of a landslide near the station are the most interesting. Owing to a landslide from the hill above, a big concrete house of Mr X had in fact llapsed on the A— Road. The following versions of this fact were recorded:

(*a*) *Many houses* have come down on the A— Road. (Exaggeration.)
(*b*) A house came *rolling down* on the A— Road. (Dramatization.)
(*c*) There was a huge landslide near CB house. Mr X's house was below. The whole thing came down.
e house was *smashed into bits*. Never could imagine such a thing. (Dramatization.)

Many reports were current to the effect that the whole Bhotia village had been wiped t. The village had, indeed, suffered very badly. Many huts, frailly built of wood and rugated iron-sheets, had collapsed. Nevertheless, although about ten persons had lost ir lives, the whole village was by no means demolished. The reports were:

*Not a trace is left* of Bhotia *busti*. (Exaggeration.)
The *whole village* has been wiped out. (Exaggeration.)
*Hundreds of persons* in the Bhotia *busti* have been buried alive. Hundreds of cattle have died. xaggeration.)
*Over fifty* Bhotias have died in the village (this figure varied from 'eight', 'fifteen', 'twenty-five' and er thirty'). (Exaggeration.)
lmost all the houses in the *busti* area have either collapsed or suffered heavy damage. It is not yet wn how many of the *busti* population have survived. (A report published on 19 June.)

[1] Bhotias are one of the hill tribes in the Himalayas.

Mr M's house had collapsed. His four daughters were rescued and had been taken the hospital. A later report says that one of them died. Mr M, with his wife and son, la buried under the debris for days and died. Many reports concerning this incident we collected. Three of them are relevant:

The *whole family* of Mr M has been killed in the landslides. (Exaggeration.)

Mr M, his wife, his son, *and all* have been killed. (Exaggeration.)

A report published on 18 June said: Mr M, his wife, and *two children* were killed in their house ne SM. in Darjeeling town. (Exaggeration.)

There had been a tragic landslide near the railway station causing five deaths. In the reports, t number of deaths varied from *five to nine.* (Exaggeration.)

Many other reports of death and damage were collected. They have been left o because the truth contained in them could not be properly verified.

*Reports about the amount of rainfall*

There were several reports about the amount of rainfall, mostly great exaggerations the 32 in. that had actually been recorded. The following are illustrations:

A student reading in the Senior Cambridge Class came on the afternoon of the 13 and said that the rainfall for Darjeeling was recorded at his school and that duri three days, 48 in. of rain had fallen. The following day we heard in the town that t rainfall had been 56 in., and that it had been 58 in. A friend said, 'Would you believe there has been over 56 in. of rain in these three days!' Various reports said that t rainfall had been 54, 46, 44, 48 and 45 in. An acquaintance said that rainfall on the 12 and 13th had been over 18 in. a day. In an article on the Darjeeling landslides, publish about a fortnight later, it was reported that there had been 36 in. of rain in three da A report, dated 15 June, said, 'The average yearly rainfall in Darjeeling area is abo 100 in., but reports available indicate that during the three worst days in the last we the rainfall amounted to 20 in'.

*Reports about the occurrence of earthquake*

There had been no earthquake shock. Nor had there been any seismographic reco of it. There were, however, many rumours concerning the occurrence of earthquake the night of the 11th. They were all pure fabrication. Earthquake rumours gain credence and became widespread some two to three days after the disaster. Only wh the rain stopped and people came out of their homes did they begin to feel that mere ra could not have been the cause of such heavy damage. In fact, belief in an earthquake the night of 11 June gradually increased. The following opinions are illustrative:

'Just see, this sort of thing (damage) was impossible without an earthquake. There certainly was earthquake. We had been asleep and did not feel it. Even the nuns who live only next door to us felt shock.' (16 June.)

I met some of the nuns and inquired. They were not very sure about the shock. One of them, howe said that she had felt the shock at about 11 p.m. But they all wondered how mere rain could cause s havoc.

Mr A, a responsible officer, narrated that somebody had told him that he had seen the news about earthquake in 'X', one of the leading dailies. He said that in that paper the earthquake had b predicted.

A European gentleman came and asked, 'Some ladies were saying that there had been an earthqu Did you feel it?' (17 June.)

A grocer told me that there had been an earth tremor on the 11th, but that he had not felt it himself. 0 June.)

A barber from the bazaar area said that there had been an earthquake. Quite a number of people spoke out it. (20 June.)

At Kurseong, a big station about 19 miles from Darjeeling, Mr V said: 'We didn't feel it, but there ust have been an earthquake. Otherwise, how could there be such cracks? Rain alone could not do it.' 0 June.)

A friend told me on the 20 June: 'There were three or four earthquake shocks on the 11th night. ou did not feel them because you were probably asleep.'

I expressed doubt to a person who said that there had been an earth tremor and that the damage had en due to it. I argued and tried to convince him that there had been no such thing. Had there been earthquake, there would have been cracks in the centre of the city as well. Since the drainage was od, and the hillsides secured with concrete slabs, rain could not do the harm in the city centre which it d caused in the outskirts. He retorted that 'earthquakes happened in *belts*, so that the whole town was t affected'. (18 June.)

*eports about the restoration of the water supply and electricity, and the conditions of road d railway*

Due to landslides, water pipe-lines had been damaged, and the water supply had been t off. The municipality had sent out notices to the effect that people should take the ecessary hygienic precautions since, for several weeks, the water supply would not be kely to be resumed. From the 12th there was no electricity. The main power-house had en badly damaged. Two days later some street lamps in the centre area were lighted, llowed, a day later, by the restoration of street lamps almost throughout the town. ut there was still no electricity for domestic use.

There had been numerous big landslides on the Hill Cart Road, which had been washed vay at various points. Some bridges on the newly laid rail-link from Calcutta to Siliguri d been damaged. Forty feet of bridgehead on the Balasan river had been washed away. rains were suspended, but were resumed on the 16th. Only one train during the day as allowed, and the speed on the track affected was restricted to five miles an hour. On e hills, however, no train or vehicular traffic was possible between Kurseong and arjeeling.

Several reports concerning all these matters had been afloat. They have been grouped gether because they are alike in being some form of 'prediction'. They are all different timates of how soon the water supply, the electricity and the road would be restored. contrast to the reports about earthquakes, which were fabrications, most of these had core of truth in them.

*ports about the water supply*

It has been announced that the water supply will be restored within a week.' (16 June.)

A responsible officer has said' (in another version, 'A friend of mine has inquired from the Municipal ice') 'that the water supply will be restored within fifteen days.' (15 and 16 June.)

Pipes have been broken at hundreds of points. It will take months before the water supply is resumed.' and 17 June.)

The pipe-line had been broken and twisted at thousands of places. The engineer had said that pipes l other implements were to be flown from Calcutta. It would take months before supply could be tored.' (16 June.)

A report, dated the 18th, said that the reporter was told by the Deputy Commissioner that 'one of the ee water supply pipe-lines was likely to be partially repaired in a day or two, making small supplies sible up to Jalapahar, from where conveyance in trucks would be arranged to the rest of the town'. ublished on 20 June.)

*Reports about electricity*

'Electricity is likely to be restored by Saturday.' (14 June.)

'The Deputy Commissioner has said that electricity to houses will be resumed within ten days (16 June.)

'Electricity has been supplied to the shops and they say that it will be restored to houses in a day o two.' (17 June.) (*Note.* This was a kind of 'goal gradient' rumour. Seeing the gradual restoration o street lamps, people thought that in gradual stages electricity for domestic use would also com Electricity for commercial purposes was intermediate in the series and was *thought of* as actually restored

'The Military have been asked to clear the debris and restore communications, water supply an electricity. They will do it in a week.' (15 June.)

*Reports about roads and railways*

'The Governor has to meet Pandit Nehru in Calcutta on the 24th. The communications will be restore by then. He has ordered the old military road to Kurseong to be made motorable by that date.' (14 an 15 June.)

'There are extensive floods in the plains. Jalpaiguri town is waist-deep in water' ('nine feet', in anoth version). 'Many bridges have been washed away on the new Assam rail-link. It will take months befo communications are resumed.' (14 June.)

'The bridge on the Balasan river has been washed away by the sudden rise in water. There is n possibility of the restoration of rail communication till before the end of the floods.' (15 June.)

A report, dated 18 June, saying that the Darjeeling-Kurseong road would not be ready before at lea a week, ruled out the possibility of the Governor's meeting Prime Minister Nehru on the latter's way bac to Delhi from Indonesia. (Published on 20 June.)

'The planters have agreed to supply the labour. Thousands of coolies are busy, and work is in rapi progress. The road will be motorable for small cars within ten days.' (17 June.)

'They have largely repaired the road near West Point. The P.W.D. contractor on the spot said that th road would be passable within 14 days; and, if they say 14 days, they will do it in less than 10 days (17 June.)

'The road is motorable up to Kurseong. The military dare not forecast the time that will be required t re-establish the road and rail-links beyond Kurseong up to Darjeeling.' (A report dated 14th, publishe on 17 June.)

'The old military road, where civil and military men were working hard, was expected to be "jeepable in about ten days. A slender supply of food would be possible along this route.' (Dated 18th, publishe on 20 June.)

'According to an official estimate, pedestrian traffic could not be resumed between Kurseong an Darjeeling before a fortnight and that for vehicular, not before four months.' (Dated 16th, publishe 18 June.)

A report, dated 18th, stated that 'The old military road was "jeepable"'. (Published on 21 Jun It was a false report.)

Relating to 19 June the following headline was seen in one of the dailies on 21 June: 'Communicatio line between Kurseong and Darjeeling opened.'

A report, dated 19 June, was published on 21 June: 'Jeep and Land Rovers reached Darjeeling for th first time since the disaster on Saturday (17 June). This road has been sufficiently renovated for sa passage of light motor traffic. It is hoped that the first supplies of about 15,000 maunds of rice and *at* (flour) sent by the Government from Calcutta will reach Darjeeling along this route this week.' (Wro report.)

'Rumours were afloat that the Siliguri-Kalimpong road was all right; and that supplies would brought to Darjeeling via the Teesta valley and Ghum-Peshok road.' (14 and 15 June.)

'That the Ghum-Peshok Road had also had slips, but might be ready in a week.' (18 June.)

'The Chief Engineer has informed the Governor that it is impossible to repair the roads for month So, a palanquin is being made for his conveyance over the affected part. I have been told by a pers who lives with the Chief Engineer.' (20 June.)

'No trains were running from Siliguri.' (18 June.)

'Trains were normal.' (18 and 19 June.)

*eports concerning sin and God's will*

Some people regarded the disaster as due to the vast accumulation of sin in the present orld. Many reports to the effect were collected, some of which are given below:

'There is too much sin in this world. We have to suffer for it.'
'Morally the present world has suffered utter degradation. You see, there have been floods, famines, ots and war in recent times that have caused widespread damage to life and property. Darjeeling had en spared so far. You will see, every town will meet with a similar fate.'
'It is all due to God's will....Can you explain the ways of God?'
'We have to suffer punishment for our sins. Worse days are ahead.'

(ii) *Increase in credulity*

Credulity had increased considerably. Statements were mainly believed without any esire to verify them. Moreover, if a fantastic statement was challenged, there was often shifting of ground. To support it, all varieties of argument were advanced, and the iscussion would generally close with the statement: 'You don't know, anything may appen.'

(iii) *'Rumour' experiment*

A 'Rumour' experiment was conducted informally on ten male subjects who were all easonal visitors to the town. Their age ranged from twenty-two to forty years. They were l moderately educated (the exact educational standing could not be determined).

The original statement, just the bare fact, was passed on to these visitors in the course f an informal conversation. They had all come to see the wreckage where the author met iem. He would move with them till eventually they transmitted the report on to other cquaintances. In fact, the experimental situation was totally informal and none of the steners suspected the author's purpose. Deviations from the original were, however, arefully noted.

The results were as follows:

The report 'A house on A— Road had collapsed' was given to five listeners, none of hom had so far seen the actual damage. Three protocols collected were:

'A house on the A— Road has been *smashed to bits* by the landslides.' (Dramatization.)
'A huge house *came rolling down* on the A— Road.' (Dramatization.)
'A big house on the A— Road had collapsed. The *whole hill came rolling* down with it.' (Exaggeration.)

The statement: 'Mr M, his wife, and his 26-year-old son has been killed' was communicated to another five listeners and was transmitted as:

'The *whole M family* has been killed in landslides.' (Exaggeration.)
'Mr M, his wife, and his son and everybody in the family have been killed.' (Exaggeration.)

(iv) *Perceptual experiment*

Panic and anxiety situations distort perception and increase suggestibility (Cantril(4)). study such distortions, the writer referred to a palatial house on a low hill about quarter of a mile away, said that *it looked crooked* and noted the replies. The experimental uation was very similar to that in the 'rumour' experiment, except that the replies ne immediately after hearing the statement. Six male visitors, varying in age from enty to thirty-five years, were the subjects for this experiment. The following are the nples of the replies:

'Yes; it does. The trees which were on the other side have fallen. It looks bare.' (Actually no tree had fallen.)

'The earth certainly has gone down. I wonder why they build houses on the very edge when there was all that flat land in front. But they will have gardens and lawns!'

'It perhaps is. But from here it is difficult to tell.'

'Yes; it does look crooked on the northern side.'

## IV. Discussion

### (i) *Explanatory purpose of rumours*

The reports enumerated above may be classified into four broad groups:

*Exaggerations with a core of truth.* Reports concerning the amount of rainfall, number of deaths, etc., come within this class.

*Pure fabrications.* These reports were purely imaginary. Rumours about the earthquake would belong to this class.

*Religious interpretations.* Reports coloured by the idea of sin, punishment and God's inscrutable ways would belong to this class.

*Predictions.* These were statements concerning the future. Reports about the re-opening of rail and road communications, the restoration of electricity and of the water supply come within this class.

The first three types, viz. exaggerated reports, religious interpretations and fabrications, represent, as Prasad (8) says, an attempt on the part of the popular mind to apprehend real facts and situations which contain a large number of unknown factors. These may, therefore, be called *explanatory* rumours. Allport & Postman (1) remark that rumours explain to the individual a baffling situation and thereby satisfy his cognitive and emotional urge to understand and comprehend. Following Bartlett (2), we may call this striving for understanding *effort after meaning*. To find a plausible reason for a confused situation is itself a motive that accounts for the vitality of many rumours. In the religious reports we may also note how culture determines the form of rumours. In India catastrophic events are often ascribed to supernatural causes and are traced to the accumulation of sin. A similar phenomenon was noted by Prasad (8) in his earthquake rumour.

On the other hand, as is evident from any of the earthquake rumours, rainfall by itself was considered inadequate to account for such widespread damage. Rainfall had been heavy. But how could mere rain water cause such cracks and havoc? This provided an area of confusion which needed an explanation. People had either experienced such damage during the earthquake of 1934, or they had heard about it because of the occasional incidence of earthquakes in this part. Naturally the idea of an earthquake was grabbed as the adequate explanation for the damage in the present case as well. On the reports about it got round, there were men to assert that *they had even felt it*. It is to be noted that suggestibility in such situations is abnormally high. Fabrications, therefore, operated in the service of meaning.

Distorted figures of rainfall are similar in principle to the distortions of proper names which are seldom, if ever, remembered with any degree of accuracy (Bartlett (2)). But why is there distortion in only one direction? With one exception, all the figures quoted are higher than the actual amount of rainfall. This is a common form of exaggeration by magnification of numbers. Allport & Postman remark: 'Seldom indeed does the numerical count of any item in rumour dwindle in telling.'

Exaggerating the figures also served an explanatory end. Even for those who accepted infall as the cause of disaster, just a few inches of rain did not appear convincing. How uld only a few inches of rain cause all that havoc? A total of 58, 56, 48, 46, 54, 45, or en 44 in. of rain in three days was a lot and something unprecedented. Such exaggerated nounts sounded more satisfactory as explanations for the vast damage.

### (ii) *Conflicting predictions*

As may be seen, reports of the *predictive* type were mainly forecasts of an early or stant restoration of traffic, electricity and the water supply. A large number of these nflicted with one another. While some visualized the resumption of services within few days, others considered them as remote possibilities.

Predictions are, as McGregor (7) says, instances of psychological inferences. Though the sychologists are not unanimous about the nature of the process of inference, they are greed upon the fact that some pre-existent attitude or set is always influential.

Social events are generally not interpreted as momentary and isolated, but are perived to be changing in one direction or another, to be 'becoming'. As Cantril (3) says: The "present" is mostly incomplete and takes meaning only when it is regarded as step between the past and the future. Numerous social situations are perpetually in the rocess of resolving themselves. As the outcome of events cannot be speeded up, it is atural that people are so much concerned with their predictions. Predictions help to ve structure to present experience, providing the individual with the psychological osures he craves.' In other words, the mechanism of *effort after meaning* which was und behind the reports of the exaggerated and fabricational types is operating.

That subjective factors should influence predictions is clear. But the degree to which ey are effective in determining the course of predictions is dependent upon the nbiguity of the external situation. Moreover, their influence varies with the importance the predictor of the predicted event. If his own welfare, pride or ideals are involved, e may expect the intensity of his wishes concerning the outcome of the situation to be eater than when the situation and its outcome are relatively remote from his personal e.

The prevalence of so many conflicting reports was due to the absence of reliable formation concerning the condition of the railway, roads, electricity and water supply. his provided a stimulus that was largely unstructured and ambiguous. The outcome of e situation directly affected the individuals. The stock of food was small and the rations d been drastically reduced. Due to the lack of proper drinking water, spread of idemic was feared. Further, most holiday-makers, who had come from the plains, sired to return home or to their duties at the earliest possible date. The degree of egovolvement was, therefore, high. Hence the situation was ideal for predictions in which bjective factors had a dominant role.

These predictions were mainly of two kinds: *Optimistic* and *Pessimistic*. Though no rect study was made of people's wishes and fears during the disaster, it may be concluded at these two classes of predictions were respectively the embodiment of individual rs and wishes. Those that forecast an early restoration of amenities belonged to the sh-fulfilment class, or were 'goal-gradient' rumours. Others were expressions of ople's anxiety concerning the predicament in which they had been placed. In other rds, they belonged to the class of 'fear rumours'.

(iii) *Main varieties of transformations*

The main forms of transformation, both in the rumours collected and in the rumou experiment were exaggeration and dramatization. Rather than report a few deaths was more striking to report a large number of deaths. 'The whole family had perished caught the attention more easily than the death of a few persons; that 'a house had com *rolling down*' produced more effect than the mere collapse of a house.

In the rumour experiment of Allport & Postman (1), 'levelling' was to be observe most frequently. In the present instance there is generally 'sharpening' by exaggeratio and/or by dramatization. This discrepancy in results can be ascribed to the difference i the motivating conditions of the two situations. Here, even the rumour experiment wa a part of the real-life situation, and the subjects were not aware of the experimenta purpose at all. In the laboratory, on the other hand, 'the reporter, feeling himself to b on trial for accuracy, tries to avoid mistakes by omitting items of which he is not sure He strives for accuracy. The atmosphere makes for caution and intended precision report. Outside the laboratory, the narrator is a spontaneous rumour agent, and, a a rule, adds colour to his story. One of his motives for telling his story is to produce a effect. That he accomplishes by exaggerating, elaborating, and dramatizing the report

(iv) *Increase in credulity and distorted perception*

In times of crisis the attitude of suspicion to any startling news, so common in ordinar life, goes into abeyance. The authenticity of 'news' is seldom scrutinized, and the desi to verify anything fantastic is absent. Cantril (4) points out that when people are ver excited, worried or afraid they are less likely to react critically than when they are calm When personal involvement is great, criticism is suspended. He observes: 'Whatev critical ability a person may normally have, it is ineffective if in any given situation h emotional insecurities are so great that they overwhelm his good judgment. Such situa tions are likely to be those where the individual himself or something dear to him a threatened. The immediacy and intensity of forces around him are such that the stimulate and sustain his low threshold of susceptibility to suggestion before his critic ability can come into play.'

The situation due to the continuous occurrence of landslides was one of great person insecurity. There was nothing effective that any individual could do for his safety. H could only wait, and with the sustained feeling that he might be the next victim Naturally, people were highly suggestible and believed in any statement, howev fantastic it may have been. The perceptual experiment also indicates that people we highly suggestible and that distortions in perception were common. Cantril (4) note similar phenomena following Orson Welles's broadcast of 'War of the World'.

Anxiety caused by insecurity acts as a *set* which affects subsequent perception of event The influence of 'set' in distorting perception of certain details of a picture has been note by Davis & Sinha (5). That rumours themselves may often serve as sets determini perceptions has been observed by Prasad (8, 9). After the Indian earthquake of 19 rumours were current that the calamity was due to the wrath of Lord Shiva because t foreigners had disturbed him in his snowy abode. The earthquake was sent as a punis ment, and Mt Everest was pushed higher up so that it became more difficult to clim After the catastrophe, the peak was visible to the inhabitants of some of the northe districts of Bihar in the clear sky of spring. People reported, when they first s

Mt Everest, that 'it had changed its shape', that 'it appeared higher', and that 'it was not as tapering and pointed as it used to be'. These cases of distorted perception strengthened and corroborated the rumours.

Such misperceptions suggest a comparison of behaviour of individuals under 'stress' with that of an 'emotionally unwell' patient. Leighton (6) in *The Governing of Men* remarks that 'Psychiatrists observing patients who are emotionally unwell have long known that when they go into a state of panic they misinterpret ordinary events as horrible threats. The whistle of a distant train becomes a death scream, or two people seen talking together are instantly assumed to be plotting.' He concludes: 'It is more than probable that this happens to otherwise normal individuals when in a state of intense fear.' It seems, therefore, that distorted perception and rumours may, under extreme conditions of stress and excitement, be an outright pathological phenomenon like the perceptions of paranoid patients.

## V. SUMMARY

The Darjeeling landslides of June 1950 produced a catastrophic situation which afforded an opportunity for studying the perception and reporting of individuals involved in it. A first-hand collection of rumours about damage, rainfall and earthquake was made. It was supplemented by some reports appearing in the newspapers. A realistic experiment was also performed to observe transformations in reporting and distortions in perception.

Transformations in reports were mainly in the direction of exaggeration, a fact also corroborated by the rumour experiment. Rumours were mostly explanatory in nature, providing reasons for the baffling calamity. Large numbers of reports conflicted with each other. They were of the type of *predictions*.

Distortions in perception appear to have been due to anxiety produced by the feeling of insecurity. Some instances from analogous situations have been recorded, and it is contended that such distortions under conditions of stress and excitement are a pathological phenomenon.

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# THE SOCIAL MOBILITY OF A GROUP OF ELDEST-BORN AND INTERMEDIATE ADULT MALES

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## I. Introduction

The data considered in this article were obtained by analysing a follow-up questionnaire completed in 1947 by 51 out of 80 miners who, between 1924 and 1939, had been awarded scholarships under a Miners Welfare Adult Education Joint Committee. These Scholarship grants of £50 per annum had enabled the men to attend a year's general cultural course at a University College for 2 days a week during term. The rest of the year the holders followed their normal coal-mining employment.

## II. Procedure

The average size of family from which the men had been drawn was 5·56. One only child having been excluded, the remaining 50 men were divided into eldests (first born not merely first-born male), intermediates and youngests. These three groups were compared as to background at the time of entry, subsequent academic achievement, and occupation in 1947, 8 years after the winding up of the scheme. It is significant that whereas the expected numbers in the groups were 9, 32 and 9 respectively, in fact there were 20 eldests, 23 intermediates and 7 youngests. Because of the small proportion of youngests it was decided to concentrate on comparing the eldests and intermediates.

## III. Comparison of eldests and intermediates

### (i) *Background on entry to the scheme*

*Siblings' occupations*

In the 1920's and 30's, mining and the equivalent female occupations were ill regarded. Most of the females in mining families were domestic servants, factory or mill 'hands'. There was general agreement that anything was better than 'pit-work', 'service' or 'factory'. Those in these occupations said this and were convinced that others also rated them as inferior. Hence any miner whose siblings were neither 'in pit' nor in 'service' or 'factory' would tend to think they had 'done better' than he. The average age of the men when awarded scholarships was 26·46; it therefore seemed reasonable to assume that the occupational tendencies of their sibs would have been shown by then and entries under 'sibs' chief settled occupations (before marriage)' would have some reference to their sibs' occupations at the time of the scholarship award. Therefore, these entries were classified according to whether all or most were mining ($M$) or not ($\bar{M}$) and/or domestic or factory ($DF$) or not ($\bar{D}\bar{F}$). The results are summarized in Table 1.

Table 1. *Distribution of sibs' occupation*

| | $M$ and $DF$ | $M$ | $DF$ | $\overline{M}$ | $\overline{D}\overline{F}$ | $M$ and $\overline{D}\overline{F}$ | $\overline{M}$ and $DF$ | $\overline{M}$ and $\overline{D}\overline{F}$ | Total |
|---|---|---|---|---|---|---|---|---|---|
| Mining | 19 | 4 | 2 | — | — | — | — | — | 25 |
| Mixed | — | — | — | — | — | 6 | 8 | — | 14 |
| Non-mining | — | — | — | 2 | 3 | — | — | 6 | 11 |
| | | | | | | | | | 50 |

*Stage of entry to scheme*

All the men had sought further education in Evening Institute or Adult Classes before seeking Two-Day Scholarships, and all except one eldest, who being physically handicapped had never worked at all, were manual workers in the coal-mining industry. Table 2 gives other data about the background of the men.

Table 2. *Scholarship men: background of family position groups*

(Expected frequency in brackets.)

| | All | Eldests | Intermediates | Youngests |
|---|---|---|---|---|
| | 50 | 20 (9) | 23 (32) | 7 (9) |
| Average age: | | | | |
| On leaving school | 13·37 | 13·2 | 13·3 | 14·07 |
| On entering mining | 13·89 | 14·2 | 13·4 | 14·6 |
| At time of scholarship award | 26·46 | 26·2 | 26·875 | 25·86 |
| Number: | | | | |
| Whose fathers were not miners | 10 | 4 | 4 | 2 |
| Who had some secondary education | 7 | 2 | 1 | 4 |
| Married at time of scholarship award | 17 | 4 | 10 | 3 |
| Unmarried at time of scholarship award | 33 | 16 | 13 | 4 |
| Whose siblings' occupations were: | | | | |
| Mining pattern | 25 | 6 | 15 | 4 |
| Mixed pattern | 14 | 7 | 7 | 0 |
| Non-mining pattern | 11 | 7 | 1 | 3 |

From this emerges the fact that the eldests left school earlier, but started pit work later, than the intermediates. The explanation may be that, in this particular coalfield, there were boys' tasks and men's tasks. Boys worked with boys and therefore were unlikely to begin pit work alongside their fathers. Eldests would tend to be kept away from the pit, but not from work, until they could look after themselves. Intermediates would tend to have elder brothers, already in mining, and deemed capable of looking after them until they could look after themselves. Hence intermediates could go to pit work straight from school. Apart from this, the eldests and intermediates differ in that the eldests are over represented and intermediates under represented in three of the categories to be found above: amongst the scholarship men; amongst those who are not in the mining sibling group; and amongst the unmarried men. These differences and their statistical significance, found by the $\chi^2$ method, may be shown as follows:

| | | Eldests | Intermediates | $\chi^2$ | $P$ |
|---|---|---|---|---|---|
| Scholarship men: frequency expected | | 9 | 32 | 4·52 | 0·035 |
| Scholarship men: frequency observed | | 20 | 23 | | |
| Scholarship men: in Mining Sibling Group: | Yes | 6 | 15 | 3·994 | 0·05 |
| | No | 14 | 8 | | |
| Scholarship men: unmarried: | Yes | 16 | 13 | 2·611 | 0·1 |
| | No | 4 | 10 | | |

*Note.* All the $\chi^2$ here and below have been corrected for continuity.[1]

[1] The writer wishes, here, to acknowledge his indebtedness to his colleague, Mr W. L. Sumner, who assisted in making these and all similar calculations.

If a value $P=0{\cdot}05$ is considered significant then the first and second of the above differences are significant and the third is suggestive.

From this evidence it seemed likely that, in the case of the eldests but not of the intermediates, there was a connexion between their having siblings who had 'done better' than they and their entering into the Scholarship Scheme.

### (ii) *Subsequent academic achievement*

There is no record of the progress of the men within the Scholarship Scheme. But, after ceasing to be Two-Day Scholarship holders, 20 of the 50 men continued to study and succeeded in obtaining either University degrees or various other recognized though lesser qualifications roughly equivalent in standard to that of a University Intermediate Examination. Table 3 shows the number of eldests, intermediates, and youngests in the three sibling occupation groups, and the number of them who secured further awards. The number who had some secondary education is given in brackets.

Table 3. *Further awards obtained by former scholarship men*

(Sibling occupation groups comprising 50 men of whom 12 obtained degrees and 8 qualifications.)

| | Number | Degrees | Qualifications |
|---|---|---|---|
| Mining: | | | |
| Eldests | 6 (1) | 1 | 0 |
| Intermediates | 15 (1) | 3 (1) | 2 |
| Youngests | 4 (1) | 0 | 1 (1) |
| Mixed: | | | |
| Eldests | 7 (1) | 2 (1) | 3 |
| Intermediates | 7 | 0 | 0 |
| Youngests | 0 | 0 | 0 |
| Non-mining: | | | |
| Eldests | 7 | 4 | 1 |
| Intermediates | 1 | 0 | 1 |
| Youngests | 3 (3) | 2 (2) | 0 |
| Totals: | | | |
| Eldests | 20 (2) | 7 (1) | 4 |
| Intermediates | 23 (1) | 3 (1) | 3 |
| Youngests | 7 (4) | 2 (2) | 1 (1) |
| All | 50 (7) | 12 (4) | 8 (1) |

It is clear that a greater proportion of the eldests than of the intermediates proved academically able. Of the eldests 55% obtained further awards, of the intermediates 26% did so. The awards obtained by the eldests were better than those obtained by the intermediates not only in being degrees rather than lesser qualifications but also in that their class of degree, where taken, appeared better. $\chi^2$ showed a suggestive difference in academic ability between the eldests and the intermediates generally, and a significant difference between those eldests and intermediates who were not in the Mining Sibling Group:

| | | |
|---|---|---|
| Eldests | 11+9 | |
| Intermediates | 6+17 | $\chi^2=2{\cdot}63$; $P=0{\cdot}10$ |
| Eldests | 10+4 | |
| Intermediates | 1+7 | $\chi^2=5{\cdot}01$; $P=0{\cdot}03$ |

Another difference that appears is in the distribution of the able eldests and the able intermediates. The former are to be found in their highest proportion in the Mixed and Non-Mining Groups, the latter in the Mining Group. That is, most of the eldests who

roved academically able had siblings who had 'done better' than they in the matter of ccupation. Indeed, it is true to say that *all* the eldests who gained further awards had iblings who had done better than they, because the one eldest in the Mining Group who ained an award had never worked at all. His siblings had clearly 'done better' than he, ven in following the ill-regarded occupations.

If we allow for this fact and separate the eldests into two groups, those 15 who had iblings who had 'done better' than they, and those 5 whose siblings had clearly not done etter, and then divide the groups so as to show, in each, how many did and how many id not secure further awards—thus 11+4 and 0+5—we obtain, applying a $\chi^2$ test, the esult $\chi^2=4{\cdot}82$, and $P=0{\cdot}037$. That is, between the two groups of eldests there is a ignificant difference in their ability to win awards. There is no such difference between he corresponding groups of intermediates.

### (iii) *Occupations in* 1947

The next comparison made between eldests and the intermediates was in terms of their ccupations in 1947, 8 years after the winding up of the Scholarship Scheme.

The results are given in Table 4. This also shows the number who in 1947 held, or had eld, elective office as members of local authorities—councillors, aldermen, etc.

We can regard the positions of mining manual worker and qualified professional man s extreme positions, those of the least and the most successful. The other positions—oreman, trade union official, unqualified professional, may be taken as middle positions.

Table 4 shows that, by 1947, 50% of the eldests and 40% of the intermediates had evered their connexion with coal mining. But, the intermediates, unlike the eldests, had ended to stay in the industry whether or not their siblings had followed mining pattern ccupations. Further, of the 10 eldests who remained in the industry, 6 were still manual orkers (immobile), whereas of the 14 intermediates only 3 were. Two of the eldests were ualified professional men whereas only 1 of the intermediates was. So, of those in coal ining in 1947, the eldests tended to fill the extreme positions of manual worker and ualified professional men, whilst the intermediates tended to fill the middle positions as oremen, and unqualified professional men, etc.

If the eldests are compared with intermediates without reference to the industry in hich they were, 15 as against 9 are in extreme positions (static or qualified professional), hereas 5 eldests as against 14 intermediates are in middle positions: $\chi^2=4{\cdot}222$, $P=0{\cdot}05$. hat is to say, there is a significant tendency for the eldests to avoid and the intermediates reach middle positions. Also the eldests tended, whereas the intermediates did not, to prove their position only if their sibs had earlier improved theirs.

This is apparent if we regard the eldests as falling into two groups; in this connexion, ose 8 whose siblings had all clearly done better than they (the 7 in the Non-mining roup plus 1 from the Mining Group) and those 12 remaining, whose siblings had not all early done better than they; and consider how many in each group did and how many d not improve their position. For the groups divide thus, 8:0, and 6:6, and, applying , the result obtained is $\chi^2=3{\cdot}6$ and $P=0{\cdot}06$. Between these two groups of eldests the fference in their ability, or inclination, to improve their position, if not significant, is early considerable. There is no such difference between the intermediates, however they e grouped. The eldests who had not moved did not appear to want to, and those who d moved had tended to reach 'top' positions and to have siblings who had earlier 'done

Table 4. 1947 *occupations of former Two-Day Scholarship Men*

| Sibling Group | Total | Remained as miners | In coal-mining industry as: Foremen | Trade union officials | Unqualified professional no further awards | Qualified professional further awards | Not in coal mining as: Foremen | Unqualified professional no further awards | Qualified professional further awards | Holders of local government office |
|---|---|---|---|---|---|---|---|---|---|---|
| Mining: | | | | | | | | | | |
| Eldests | 6 | 4 | 1 | — | — | — | — | — | 1[1] | — |
| Intermediates | 15 | 3 | 2 | 2 | 1 | 1 | 2 | — | 4 | 4 |
| Youngest | 4 | — | — | 1 | 1 | 1 | 1 | — | — | 2 |
| Mixed: | | | | | | | | | | |
| Eldests | 7 | 2 | 1[2] | — | — | 1 | 1[2] | — | 2 | — |
| Intermediates | 7 | — | 2 | 2 | 1 | — | 1 | 1 | — | 4 |
| Youngests | 0 | | | | | | | | | |
| Non-mining: | | | | | | | | | | |
| Eldests | 7 | — | — | — | — | 1 | 1 | 1 | 4 | — |
| Intermediates | 1 | — | — | — | — | — | — | — | 1 | — |
| Youngests | 3 | — | — | — | — | 1 | 1 | — | 1 | — |
| Totals: | | | | | | | | | | |
| Eldests | 20 | 6 (30%)[3] | 2 | — | — | 2 (50%)[4] | 2 | 1 | 7 (45%)[5] | — |
| Intermediates | 23 | 3 (13%)[3] | 4 | 4 | 2 | 1 (61%)[4] | 3 | 1 | 5 (26%)[5] | 8 |
| Youngests | 7 | — — | — | 1 | 1 | 2 (57%)[4] | 2 | — | 1 (43%)[5] | 2 |

[1] Had never worked and properly belongs in Non-mining Sibling Group.
[2] These 2 obtained further awards but alone of those who did so failed to become professional men.
[3] Percentage static. [4] Percentage in mining.
[5] Percentage qualified professional including those from coal-mining industry.

better' than they. The intermediates all appeared to want to move and nearly all actually did move into middle positions.

Finally it may be noted that *no* eldests were to be found amongst those who in 1947 held, or had previously held, elective office as councillors and aldermen in local government bodies, who, incidentally, include in their number all the full-time trade union officials. Of the intermediates 8 were councillors, etc., and 15 were not, whilst of the eldests 0 were councillors, etc. (and 20 were not). $\chi^2 = 8{\cdot}385$, $P = 0{\cdot}01$, which is statistically highly significant.

### (iv) *Summary of findings*

The main results of the three-fold comparison can be summarized as follows:

| | $\chi^2$ | $P$ |
|---|---|---|
| 1) Eldests were over represented and intermediates under represented amongst the scholarship men | 4·52 | 0·035 |
| 2) The eldest tended and the intermediates tended less: | | |
| (i) To be unmarried men | 2·611 | 0·1 |
| (ii) To have siblings who had 'done better' than they | 3·994 | 0·05 |
| (iii) To be academically able | 2·63 | 0·1 |
| To be academically able if their siblings did not follow mining pattern occupations | 5·01 | 0·03 |
| (iv) To avoid middle occupations and either to stay in one or attain the other extreme occupational position | 4·222 | 0·05 |
| 3) Eldests completely avoided local government elective office and intermediates did not | 8·385 | 0·01 |
| 4) In the case of the eldests, but not in that of the intermediates, there appears at all three points a significant connexion between their achievements and those of their siblings: | | |
| (i) They were over represented amongst the men who had siblings who had 'done better' than they | 3·994 | 0·05 |
| (ii) The eldests who had siblings who had 'done better' than they showed the greatest academic ability, the other eldests showed none | 4·82 | 0·037 |
| (iii) The eldests whose siblings had not all clearly 'done better' than they showed the greatest inclination to remain mining manual workers, the other eldests showed none | 3·6 | 0·06 |

It is clear that there were significant differences between the eldests and the intermediates, and these will later be examined. But the difference that occurs at every point is that, in the case of the eldests only, their activity appeared related to, and seems, almost, to have been determined by, the activity of their siblings, whereas the activity of the intermediates was not. Once they had entered the Scholarship Scheme, the eldests seem to have been inclined to 'get on' only if their siblings had already done so; the intermediates all either 'got on', or wanted to do so, whether their siblings had or not.

So the question arises: Is it likely that eldests are more affected than the intermediates by the activity of their siblings, and if so why?

## IV. The attitude of the eldests

is intended first to consider the question, why should adult male working-class eldests be likely to be affected by the achievements of their siblings; and then to examine in more detail the facts relating to the eldests amongst the scholarship men.

### (i) *An hypothesis*

Are eldests likely to be affected by the activity of their siblings? The following discussion should not be taken as factual but as speculation, based, however, on observation.

There is little doubt that in working-class families eldests in childhood and adolescence have to bear more responsibility than their siblings. Also they have to be aware of tensions and strains in the family, to mediate between the adult and child worlds and generally to sharpen and develop their capabilities. This tends to set them apart from the others, and to encourage a feeling of 'difference' from and soon, probably, some superiority to 'the kids'. As a result of family position and experience they are likely to become more individualistic, better fitted for competition, scholastic or other, than their siblings. Nevertheless, they are far too often compelled, in part by their developed sense of responsibility, to forgo, if not to sacrifice, opportunity in order to meet family needs they know to be real. They tend to leave school at the earliest possible moment and to find themselves at work, under the compulsion of conscience and family, augmenting the family income. So their difference from their siblings is reinforced and also their conviction of superiority. This in turn may lead to unconscious resentment of any movement of the others towards equality. If any such movement occurs, e.g. when siblings by work or, by longer stay at school, begin to do what the eldests did (or might have done), the latter are likely to be critical and to justify their resentment by referring introspectively to their own past. Thus they may be tempted on the one hand to conjure up a past of 'shouldered responsibilities' or 'opportunities forgone' in order to confirm that they were *then* superior to their siblings, as those seem now. Alternatively, their belief in the future they had, and in the virtue which went unrewarded in their past, may be used to support a conviction of continued superiority. Thus by their early twenties, eldests may tend to develop a strong inclination to 'stay put', and they will do that unless something disturbs their feeling of difference and superiority, and hence their comfort and satisfaction. If their siblings do not very obviously 'do better', then eldests may be able to continue to feel 'natural' ('different', 'superior', 'comfortable')—and having no inclination now for movement, may possibly lose the capacity for it. If, however, their siblings *obviously* 'do better', a conflict will be set up between their view of themselves, as natural superiors, and that of the external world, which will rate them *inferior* to their successful siblings. In so far as they are aware of and share the view of the world they will tend to feel uncomfortable, unnatural, unlike 'themselves'. In these circumstances they will recognize subconsciously that they must take action. They may either cut adrift from their siblings, for example by getting married, and so escape unfavourable comparison or do something clearly different from, and if possible better than, anything they or their siblings have yet done. Only thus will harmony of feeling be restored. If they cannot cut adrift they must do something else. But, having a developed *dis*inclination for movement, they will resent the necessity for it, and be anxious to do enough and no more. Their desire will be to feel 'natural', to be themselves again, it will not really be to 'get on'. So if their siblings have 'done better', more in the resentful, jealous, view of the eldests than in the view of the outer world, which for example does not distinguish between 'colliers', a demonstration of difference, such as getting 'to College' will, perhaps, suffice as proof of superiority and also to restore the natural order of feeling. But if their siblings have clearly 'done better' in external opinion as well as in the view of the eldests, the mere demonstration of difference may not suffice to restore comfort to the eldests. Hence further effort, which once embarked on may become habitual, will be necessary, not for the sake of 'getting on' but in order to establish harmony of feeling.

(ii) *Verifying expectations derived from the hypothesis*

In considering the group with which we are specially concerned, it is important to tress that, with one exception, they were miners, almost all in their early twenties, and, ince mining was an ill-regarded occupation, not as dependent as other eldests might be n their own judgement as to whether their siblings had 'done better' or not than they. 'urther, in seeking and in achieving entry into the Scholarship Scheme, they were doing most unusual thing; miners did not go 'to College'. Hence, specially in view of their ge, we would expect from the eldests not only disinclination to move from the family-ocial milieu, but also that:

(1) They had moved into the Scholarship Scheme *because* they had siblings who had done better' than they.

(2) They had moved into the Scheme as unmarried men, *because* married men would e less sibling conscious and more likely to have fled from sibling competition.

(3) Those would henceforth tend to be immobile whose siblings had *clearly not* 'done etter' than they. For these eldests would have 'proved' they were still different from nd superior to their siblings by obtaining scholarships 'to College'.

(4) Those would tend to be most mobile whose siblings *had clearly* 'done better' than hey. For these, not having proved their continuing superiority merely by obtaining cholarships, would feel driven to prove that they were better than 'colliers' in order to how they were superior to their siblings.

(5) Those whose siblings had *not clearly* 'done better' than they would show signs of ndecision and be subsequently either immobile or mobile. They would not be sure whether y getting 'to College' they themselves had 'proved' their superiority or not.

As reference to the tables will show, the facts assembled accord well enough with the bove expectations, as they should since the hypothesis itself was derived from them. ut it is worth considering the twenty eldests in greater detail to see if closer analysis f the facts confirms the main hypothesis.

The results of closer analysis can be summarized as follows:

(1) Fourteen eldests, those not in the Mining Sibling Group, had siblings who had lone better' than they occupationally. In the Mining Group, however, there were 3 who r physical reasons (including one case of accident) had not become coal-face men, 'real olliers'. Two of them had had to take the place of father to younger siblings and had ot cut adrift by marrying. Hence 17 out of the 20 eldests may be said to have had ore successful siblings.

(2) The tables show 16 eldests to have been unmarried: but actually one, given as arried, had, before entering the Scholarship Scheme, become a widower and had turned to the home of his siblings, some of whom had 'done better' than he. So 17 of e 20 were, effectively, unmarried.

(3) The tables show 6, in the Mining Group, whose siblings had *clearly not* 'done better' an they. From these must be deducted one *A*, who because of physical reasons had ver worked at all. Of the remaining 5, 4 proved quite immobile (and apparently ntent to be so) as mining manual workers and 1 proved nearly so, having moved only become a mining foreman.

(4) Seven in the Non-Mining Group had siblings who *had clearly* 'done better'. If these is added *A*, deducted from the Mining Group, there are 8 eldests of whom

6 became qualified professionally, 1 an unqualified professional man and 1 a non-mining foreman. All these in fact proved more successful than their siblings, and, as a group more successful in every way than any other group of scholarship men.

(5) In the Mixed Group there are 7 whose siblings *had not clearly* 'done better' than they Of the 7, 2 remained mining manual workers, 1 became a mining and 1 a non-mining foreman, and 3 became qualified professional men. That is to say, there is the 'mixed attainment on the part of these eldests which was anticipated.

But the detailed evidence also suggests that the 3 who became qualified professiona men had, in addition to the motive for movement which we have attributed to eldests a conscious desire to be other and better than 'pit men', and to satisfy this merely havin been 'to College' was not enough. Adding these 3 to the 8 with successful siblings it ma be said that of the eldests only those 11 to whom the obtaining of a Two-Day Scholarshi could in itself *prove nothing* were subsequently socially mobile, most of them highly so the remaining 9 were subsequently immobile or relapsed into immobility. So the detailed evidence does accord with the suggested importance of the eldests' siblings' positions.

### (iii) *The eldests' avoidance of elective office*

It remains now to consider why, when not all eldests do so, these eldests, both befor and after they were Scholarship men, avoided local government office.

One possible explanation is that, entering the Scholarship Scheme primarily to restor their position of being in advance of their siblings, they were content, if this metho succeeded, not to be councillors. And if their superior position was not thereby restored they found themselves far from the very coalfields where elective positions might hav been open to them. It is also possible that the role of eldest discourages attraction t holding office, the obtaining and keeping of which depends so much upon the effort an good opinion of others. These eldests felt they could restore themselves in their ow judgement *only* by their own efforts, small though they might hope these would have t be! Indeed, and this is further confirmed by personal acquaintance with some of th people concerned, their disinclination for unaccustomed social movement seems to b linked with dislike of effort except it be of their own free choice, and with attraction t a past wherein a distinctive position was theirs by nature without a struggle, so they onl had to please themselves. Not surprisingly, if this were so, they would want thei existence as much as possible under their own independent control, neither to be com mitted in their own lives by the decisions of others, nor to feel that they were battenin on other people's effort. In other words, they were individualistic, self-and-siblin regarding, and they had developed a common attitude of consulting, pleasing and relyin upon themselves.

A common attitude on the part of the intermediates was more difficult to identify, bu that there was one, and the nature of its operation, we shall now attempt to show.

## V. The intermediates

So far, the evidence suggests differences between the intermediates and the eldests, th chief difference being that whereas the behaviour of the intermediates seemed uniforml unrelated to that of their siblings, the behaviour of the eldests was not. We have als suggested that the apparently contradictory behaviour of the eldests resolved itself int consistent 'eldest' behaviour.

(i) *Evidence for a common attitude*

It was first considered whether the intermediates had, like the eldests, anything in common at the time of their entering the Scholarship Scheme. Analysis of the data showed that 20 of the 23 had recently found themselves in new, and for them unusual, positions, and seemed to have become scholarship men with a view to meeting the responsibilities of these positions. At the time of their scholarship awards, 10 were married, in most cases fairly recently, and 9 of them had, or were about to, become fathers: another 5 had recently experienced the calamity of accident or full *un*employment. Another 5 had friends whom they either emulated in becoming, or 'ganged up' with to become, scholarship men. So far as is known, none of the eldests had recently married or sustained injury or 'ganged up' with friends for scholarships, and only one, and he was married, mentioned full unemployment.

So the intermediates were, in the main, in different circumstances from the eldests and in situations which, however common, were new, unusual, individual, as far as they were concerned. Whilst it seemed true that the eldests had desired to restore a former position it seems equally true that the intermediates desired to change and improve their position.

The next items considered were the academic ability and achievements of the intermediates. Only 6 went on to obtain further awards and these seem to have had little natural aptitude or inclination for academic activity. Although three ultimately took degrees and three secured qualifications, in four cases these degrees and qualifications took longer to get and they were *all* inferior to those taken by the eldests or youngests. All this points to the intermediates having had even less natural inclination than ability for academic activity, which, it is relevant to remark, is individual activity.

Finally, the occupational and social activity of the intermediates was reviewed. They had all tended to 'get on' and certain facts, shown in the tables, give some indication as to how they 'got on'. They engaged in activity, which eldests did not, as elected members of local authorities (35 % of them were councillors or aldermen), and as full-time trade union officials. These facts suggested that they were more likely than the eldests to engage in group activity, and scrutiny of the replies revealed not only that they were more prone to such activity than eldests, but also that they had, in the main, 'got on' in and through their activity in connexion with groups.

In general, they had become foremen as a result of being active in trade union, pit, welfare and other committees, unqualified professional men, e.g. holders of minor administrative posts, as a result of being active trade unionists or local politicians, and *established* as qualified professional men as a result of being 'local boys' or having been 'pit lads'. In short, the evidence suggests that, whilst the eldests were self-and-sibling-regarding, the intermediates were other and other than-sibling-regarding.

(ii) *Why may intermediates have an other and other-than-sibling regarding attitude?*

Intermediates were never eldests: they were once youngests in the family. They were never like the eldest, but they had been, until another sibling came along, in a different position of prominence as youngest and, in that position, had been, not perhaps a most, but certainly a very, important person. They were not, and never could be, one kind of important person; and when another sibling appeared they were no longer very important persons: this seems to be the fundamental fact.

They came to occupy their family position after occupying a more important position,

and as a result of circumstances beyond their comprehension or control: this is true of all intermediates. In their position, they are soon likely to feel that they are entitled to more consideration than they get, because hitherto they have had more, and also that they are perhaps somewhat 'battered victims' of circumstance. Hence they are likely to feel rather 'hard done by' and somewhat apprehensive, and to want to improve their position—but to be aware of forces arrayed against them.

However, once in the position, they find it has certain advantages. It is sheltered, so that when family storms blow, those in other positions, particularly that of eldest, are more likely to have to brave the force of circumstance. Meanwhile, intermediates are apt to escape attention, or, at least, the close attention given to those in extreme positions, to be left more to their own devices, and to be able more to do as they like. They will not perhaps, know very well what they do like; but they will know better what they do not like, and they will have more opportunity of evading that.

The position also has its disadvantages. It is safe and sheltered compared with other positions; but the shelter and safety of it, in storms, may serve to emphasize their force when they come and give rise to helpless apprehension of such circumstances. Intermediates may escape attention; but attention may be pleasant as well as unpleasant, and if they are liable to escape the unpleasant, they are also liable to escape the pleasant attention. If they are more able than those in other positions to do as they like, the condition is that they allow themselves to be 'shoved around'; whilst not doing what they do not like results in more apprehension, and in being beholden to others who do for them what they ought to do.

The advantages and disadvantages of their position arise from its being a less exposed or important position, a less responsible position than that of eldest and a less privileged than that of youngest; and both the advantages and disadvantages serve to emphasize, to intermediates, the importance of others, and their own powerlessness to control, though not to derive benefit from, the movement of events.

If this is true of the position of intermediates, then, as a result of their experience of the position, they are likely to develop two different inclinations, irreconcilable within the family, and so a contradictory attitude towards the family, and, perhaps, also a general attitude of indecision. On the one hand they will want to be sheltered. On the other they will want to be important, for they were once so. These inclinations, both to stay in the group and to 'get on', developed within the family, are irreconcilable within it; for whilst it provides shelter it cannot provide opportunity for improvement of their position. In order to reconcile their inclinations—to avoid complete frustration—the intermediates must turn to the world outside, to others beyond the family.

What happens at this point clearly depends, much, upon the social and geographical position of the family. The inclinations of intermediates will lead them to attach themselves to a group, in which they will feel safe, as in the family. But, because of the geographical isolation or the superior social position of their family, there may be no such group available. In this case it seems probable that they will attach themselves to objects or animals or, if they can, to individuals; and, gaining a feeling of significance in relation to such objects and creatures, they may come to feel confident, without being in a group and cease to fear to walk alone. However, working-class families, and particularly such families in mining towns and villages, in which all the intermediates who were scholarship men were brought up, are not isolated, as a rule, either socially or geographically. Those

ving in such families do, in fact, find it easier to attach themselves to groups than to ıdividuals. So, in such circumstances of proximity and rough equality between families, ıtermediates, who to 'be themselves' must reach outside their family, find, ready to eceive them, some group or gang in which they feel safe and also *un*inferior.

Intermediates can turn to account their experience of having to 'fit in' with others, hey may tend to be effective and to find satisfaction in group activity, and to continue ı it if they can. So they will tend to lose, if they ever had either, both inclination and ptitude for individual activity. They will tend to develop the initiative of a member of team rather than that of the captain of the team; to await opportunity, rather than eek opportunity; and, generally, to accommodate themselves to others rather than have thers accommodate themselves to them. Withal, they will continue to want to move, o 'better themselves', to be important, and to 'get on'. But they will want to move in nd with their group; not alone. So they will not move quickly, or far.

If this general view about intermediates is correct, then the behaviour of intermediates ı relation to the unique Two-Day Scholarship Scheme and of those who passed through he Scheme becomes intelligible. They avoided the Scholarship Scheme because it was inusual. Those who entered it did so because they were influenced by others whom they ield in high regard, or by calamity; not because they had been influenced by the activity f their siblings. And, as scholars, they did not do very well, because they had neither ıclination nor aptitude for individual activity. Subsequently, however, they all improved heir position because they wanted, and felt they ought, to improve their position. They id so, in and through group activity, because they were attached to groups and had cquired liking and aptitude for group activity. Finally, they won the approbation of roups or (as councillors and aldermen) of communities, because they were inclined to eek such approbation, and inclined to need the evidence of such approbation in order o feel that they counted.

## VI. Conclusion

tatistically significant differences were found between the eldests and the intermediates, nd between groups of the eldest, and not between groups of the intermediates. It eemed that the behaviour of the eldests was connected with that of their siblings, and ıat the behaviour of the intermediates was not so connected.

The apparently contradictory behaviour of the eldests seemed explicable on the ypothesis of an eldest self-and-sibling regarding attitude.

That of the intermediates was more intelligible on the assumption of an other and other-ıan-sibling regarding attitude. Evidence of both attitudes was adduced.

The eldests and the intermediates were, at the time they came within the range of this quiry, adult males: and if their behaviour is rendered intelligible in terms of attitudes eveloped in the family, as a result of contact with their siblings, there is at least a esumption in favour of the view that the behaviour of other eldests and other inter-ediates may be susceptible of explanation in similar terms. If this problem is to be vestigated there will need to be found, either another occupational group such as miners nstituted in the 1920's and 1930's, convinced that their occupation caused them to be ted as inferiors, or some means of testing whether adult male eldests and intermediates ffer in their general attitude and outlook.

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# PERCEPTUAL ANTICIPATION IN TRACKING WITH TWO-POINTER AND ONE-POINTER DISPLAYS

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I. *Introduction* (pp. 222–223). II. *Speed of course* (pp. 223–224). III. *Practice* (p. 224). IV. *Size of display and response movements* (pp. 224–225). V. *Response time* (pp. 225–227). VI. *Smoothness* (pp. 227–228). VII. *The task with two pointers and with one pointer* (p. 228). VIII. *Summary* (pp. 228–229).

## I. Introduction

This paper has been written to contrast a two-pointer display in tracking with a one pointer display, and to demonstrate the role which anticipation plays with these two types of display. The experiments are an elaboration of an unpublished experiment carried out by K. J. W. Craik and M. A. Vince in 1945 to study anticipation in tracking. In two-pointer matching the subject had, by means of a positional control, to keep a pointer in line with a second pointer, which moved in a single vertical dimension in a simple harmonic or more complex harmonic course. In one-pointer balancing the subject could only see a single pointer, which was moving in a single vertical dimension as before, and which he had to keep on a fixed line. The experimental arrangement is shown in Fig. 1. The subject always held the response handle or wheel so that an upward response movement raised the corresponding display pointer.

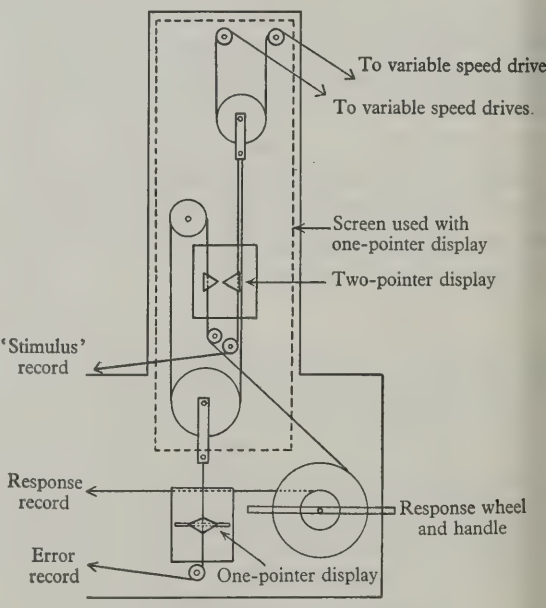


Fig. 1. Experimental arrangement.

Anticipation can be said to occur when a subject either prepares to make a particular response before the arrival of the appropriate signal, as in the fore-period of the classical simple reaction experiment, or when he actually responds before the signal arrives, as in the classical premature reaction. For purposes of experimental study, anticipation can be divided into two kinds, according to the source of the information upon which the subject acts. When the information is presented to him in advance, his use of this advance information will be called receptor anticipation. An example is any spatially extended display which is visible for a certain distance ahead of the point at which responses have to be made.

When, on the other hand, no advance information is given, but the subject is able to deduce the nature of the future signals from his past experience, his use of the

nformation will be called perceptual anticipation. An example is the classical premature reaction, which occurs when the subject has learnt the time interval between the warning signal and the signal to which he has to react. It is only this latter kind of anticipation which can occur in the tracking task described here.

## II. SPEED OF COURSE

In one experiment a comparison was made between the two-pointer and one-pointer displays with different speeds of the cams producing the 'stimulus' course. A Latin square design was used, with 16 subjects. The mean size of the error over 32 half minute periods, expressed in units such that 100 would have been the subject's error had he not responded at all, has been plotted against speed of course in Fig. 2.

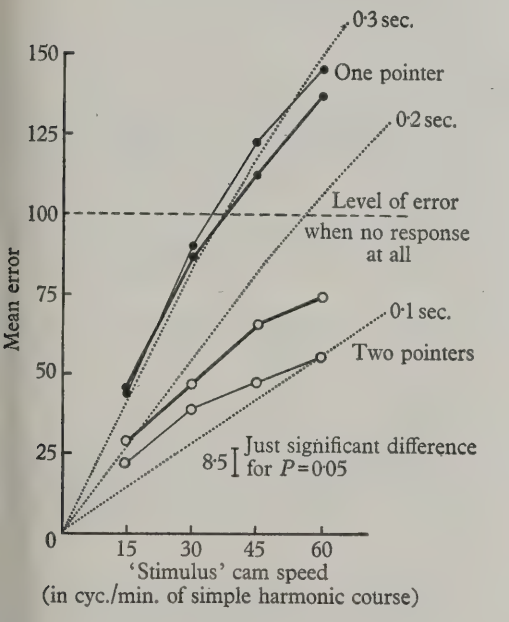


Fig. 2.

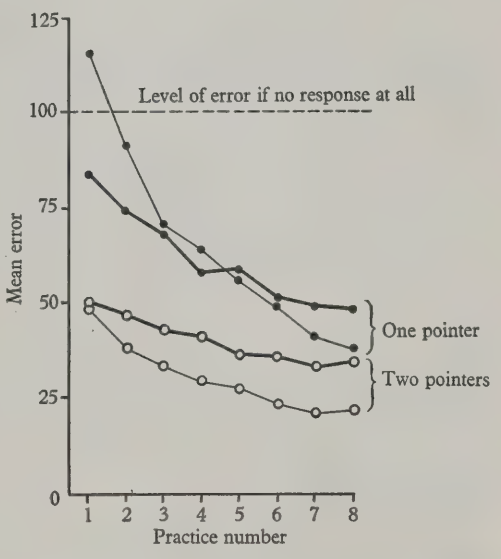


Fig. 3.

Fig. 2. Mean error in tracking harmonic courses, produced by cams of a constant size running at different speeds. This is shown separately for each of the two kinds of tracking task and 'stimulus' course. The lower two continuous curves are for two-pointer matching, the upper for one-pointer balancing. Thin lines: simple harmonic course. Thick lines: more complex harmonic course. The speed of the cams producing the 'stimulus' courses has been expressed in cycles per minute of the simple harmonic course, and of the larger of the two harmonic displacements of which the more complex harmonic course consisted. The smaller of the two displacements of the more complex course was half the size of the larger, and 1·37 times as fast. For 5% significance, differences must be 8·5 or greater. The horizontal broken line gives the level of error which would have occurred with the constant sized cams had the subject not responded at all. The sloping dotted lines give the approximate error which would have occurred had the subject responded on the average 0·3, 0·2 and 0·1 sec. off course.

Fig. 3. Learning curves for the four different combinations of two kinds of display, and two 'stimulus' courses. The upper two curves are for the one-pointer display, the lower for the two-pointer display. Thin lines: simple harmonic course of 32·5 cyc./min. Thick lines: complex harmonic course produced by four equal-sized displacements of 32·5, 25, 22·5 and 14 cyc./min. For 5% significance, differences must be 12 or greater in the first practice, 5 or greater in the eighth practice. The broken line gives the level of the error which would have occurred if the subject had not responded at all. This has been taken as 100.

Fig. 2 shows two important differences between the two-pointer and one-pointe displays, both of which are related to anticipation. First, at all display speeds the erro with two pointers was nearly half that with one pointer. The subject averaged betwee 0·1 and 0·2 sec. off course with two pointers, as compared with 0·3 sec. with one pointer At the two fastest display speeds he would actually have produced a lower error scor with one pointer by doing nothing than by attempting to respond.

Secondly, with the two-pointer display, the mean error was smaller with the completel predictable simple harmonic course, than with the less predictable more complex harmoni course. (It will be noted that this comparison was made by expressing the mean error with each course as proportions of the error which would have occurred with the cours if the subject had not responded at all.) Whereas with the one-pointer display there wa no significant difference between the two courses at the two slower speeds, the only tw speeds at which the subject's attempts at correction proved at all successful.

## III. Practice

In a second experiment each of 12 subjects received 8 practice periods of 16 min. each spread out over a week. Half of each period was with the two-pointer display, and hal with the one-pointer display. In each half both a simple and a complex harmonic cours were used. The order of displays and courses was arranged in a Latin square. An attemp was made to keep motivation high, in order to make the rate of learning as rapid a possible. The learning curves are shown in Fig. 3.

The means shown in Fig. 3 for the second practice, correspond closely to the mean shown in Fig. 2 for the 30 cyc./min. courses. Although the majority of the improvemen which occurred was in performing with the one-pointer display, yet the two-pointe display was always significantly more accurate than the one-pointer. Most of th improvement was in fact due to the extremely low initial standard of performance wit the one-pointer display. (With one pointer the mean error for the two courses combine was the same initially as the error which would have occurred if the subject had no responded at all.)

Taking the subject's performance with the one-pointer display as the standard fo making comparisons, it will be seen that in the first practice period with the completel predictable simple harmonic course, there was a saving of 60% in the size of the mea error when the two-pointer display was used. In the eighth practice period there was sti a saving of 40% when using the two-pointer display. With the less predictable comple harmonic course, there was a saving of 40% when using the two-pointer display in th first practice period, which was reduced to 30% in the eighth practice period.

It is to be concluded that practice with the one-pointer display can to some exten compensate for the deficiencies of the display, especially with a regular predictable course Yet even after practice with both displays, the mean error can be reduced by about 35% by changing from a one-pointer to a two-pointer display.

## IV. Size of display and response movements

In comparing the results of two-pointer matching and one-pointer balancing, it will b seen from Fig. 1 that although the size of the response movements was identical in bot cases for any particular size of the cam producing the 'stimulus' course, the display siz

vas halved in one-pointer balancing as a result of the arrangement of pulleys. A third xperiment was therefore performed on 12 subjects, using the same sizes of display and esponse movements as those used in the present series of experiments, to determine the ffect of variations of this order in the size of the display and response movements. A Latin square design was again used.

The results given in Table 1 show that in both two-pointer matching and one-pointer alancing, neither variations of this order in the size of the display movement, nor in the ize of the response movement, had any significant effect upon the error resulting from racking with a constant sized cam. It will be seen that the only significant differences vere between the two-pointer display and the one-pointer display.

Table 1. *Mean error in tracking a harmonic course from a cam of constant size, for different sizes of display and response movements*

(This is shown separately for two-pointer matching and one-pointer balancing, in each case for two sizes of isplay and response movements. The size IV display was twice the size II, and four times the size I. Similarly ne size 2 response movement was twice the size 1. As both the simple and the more complex harmonic courses sed showed similar results, these have been combined. The level of error which would have occurred with a onstant-sized cam had the subject not responded at all, has been taken as 100.)

| Size of the required response movement | Nature of the display: Two pointers, Size IV | Two pointers, Size II | One pointer, Size II | One pointer, Size I | 5% point for differences |
|---|---|---|---|---|---|
| 2 | 20 | 19 | 37 | 36 | 5 |
| 1 | — | 22 | — | 37 | |

## V. RESPONSE TIME

n order to determine the extent of anticipation with two pointers and with one pointer, neasurements were made of the subject's response time. This was taken as the time etween a change of direction in the movement of the 'stimulus' course, and a corre- ponding change of direction in his response movement. Sample distributions of response imes at the start and end of the learning curve experiment (see p. 226), are shown n Figs. 4 and 5. Fig. 4 shows the changes resulting from practice in tracking the mple harmonic course with the two-pointer display. Fig. 5 shows the corresponding hanges with the one-pointer display.

It will be seen that with the two-pointer display the subject learnt during the eight ractices to improve the timing of his matching of the completely predictable simple armonic course. In the final practice his response time distribution to changes in the irection of the 'stimulus' movement showed less scatter than initially. And he was, if nything, more often ahead of the 'stimulus' than behind it, when matching its changes direction.

The corresponding distributions for the less predictable complex harmonic course were general similar, except in two respects. First, both distributions had a significantly rger tail to the right (negative) side, so that the overall scatter was in both cases larger an with the simple harmonic course. And secondly, although the tail was smaller after actice, the distributions before and after practice were not significantly different by at the 5% point. Evidently the subject tended to lag farther behind with this course, d could not significantly improve his timing.

It is to be concluded that, with the two-pointer display, the improvement in pe formance shown in Fig. 3 with the less predictable complex harmonic course must ha been due largely to increased manual dexterity in the use of the experimental apparatu accompanied by a raised standard of what the subject would accept as 'alinement Only with the completely predictable simple harmonic course could the subject al improve the timing of his matching movements.

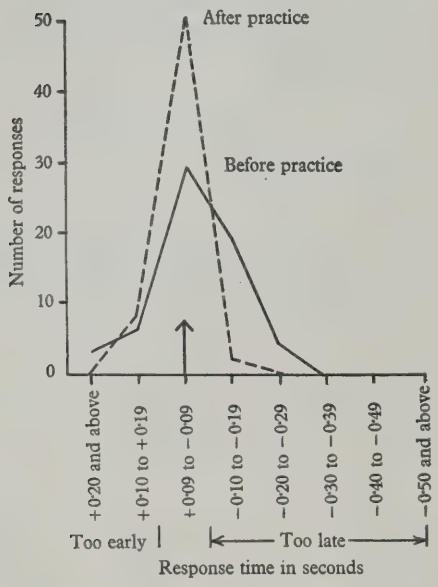


Fig. 4.

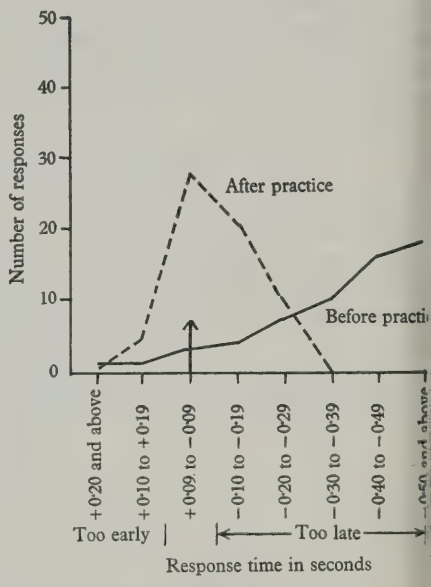


Fig. 5.

Fig. 4. Distributions of response times in two-pointer tracking with the simple harmonic course. Continu lines: start of first practice. Broken lines: start of last practice. The two distributions are significan different by $\chi^2$ at the 1% point.

Fig. 5. Distributions of response times in one-pointer tracking with the simple harmonic course. Continu lines: start of first practice. Broken lines: start of last practice. The two distributions are significan different by $\chi^2$ at well beyond the 0·1% point.

In contrast, with the one-pointer display, the subject learnt to improve the timing his balancing of both the simple and complex harmonic courses. However, after pract with the predictable simple harmonic course, his response time distribution to changes the direction of the 'stimulus' movement showed significantly more scatter than w the two-pointer display (see Fig. 5), and his responses lagged behind significantly. W the less predictable complex harmonic course the residual scatter and lag after pract were significantly more marked still, although the initial distributions were more or l similar for the two courses.

It follows that some of the improvement in performance shown by the one-poin display learning curves in Fig. 3 may have been due to increased manual dexterity, a the associated raised standard. But a great deal of it was evidently due to the impro timing of the subject's balancing movements.

Comparing Figs. 4 and 5, it will be seen that both initially and after practice the sub

gged significantly farther behind with one pointer than with two pointers, when tracking he simple harmonic course. Similar highly significant differences were found with the mplex harmonic course.

If the subject had not anticipated the changes in the direction of the 'stimulus' movement, the changes in the direction of his response movements would inevitably have ccurred at least 0·2 sec. behind them. Whenever the subject responded earlier than 2 sec. after the corresponding change in the direction of the 'stimulus' movement, he ust have been initiating his response before the change occurred. Thus, initially, the ubject was anticipating most of the time with the two-pointer display, and sometimes ith the one-pointer display. Practice with the two-pointer display resulted in his arning to anticipate still more accurately with the completely predictable simple armonic course. Practice with the one-pointer display resulted in the subject learning anticipate most of the time with both the simple and complex harmonic courses. owever, as compared with two-pointer matching of the simple harmonic course, mplete anticipation was not learnt with the less predictable complex harmonic course, ven with the two-pointer display. Nor was complete anticipation learnt with the efficient one-pointer display, even with the completely predictable simple harmonic ourse.

## VI. SMOOTHNESS

hen the subject is performing accurately, smoothness is a further measure of anticipaon. When no anticipation is possible, wandering off course for at least one reaction time inevitable. And if the subject is following accurately, he must return to the course as apidly as possible every time he wanders off it. Thus accurate tracking without anticipaon consists in a series of rapid jerky corrections.

However, smoothness is a less sensitive measure of anticipation than response time. is possible for the subject to track smoothly without anticipation, by sacrificing ccuracy and simply following at least one reaction time behind the course. And again lse anticipations, followed by the corresponding corrections, increase jerkiness. Yet they nd to occur with less easily predictable courses when the subject is showing a conderable degree of successful anticipation. (Just as a premature reaction in the classical mple reaction experiment becomes a false reaction on the 'catch' episodes.)

The smoothness of tracking at the start of the first and last practice periods was stimated by counting the number of unnecessary discrete changes of speed made. The wer the number of such unnecessary changes, the smoother performance was taken to be.

It was found that initially with the simple harmonic course the subject performed gnificantly more smoothly with two pointers than with one pointer. There was no fference with the complex harmonic course. After practice, performance with the mple harmonic course had become significantly more smooth with both displays, two ointers still maintaining a significant superiority over one pointer. There was a nongnificant improvement also with the complex harmonic course in two-pointer matching. ut there was no difference whatever with this course in one-pointer balancing.

It is to be concluded that by the criterion of smoothness of performance, the subject owed more anticipation initially with the predictable simple harmonic course in twointer matching than in one-pointer balancing. Practice increased the amount of ticipation with this course, the two-pointer display showing more anticipation than

the one-pointer display. However, smoothness was not a sufficiently sensitive measure anticipation to show significant differences with the less predictable complex harmon course.

## VII. The task with two pointers and with one pointer

Two-pointer matching has two distinct advantages over one-pointer balancing, which a a direct result of the difference in the nature of the display. These two advantages a held to account for the differences in the results with two pointers and with one pointe which have been described above.

In two-pointer matching, first, the subject can see the 'stimulus' movement. An secondly, he can see the direct effect of his response movements upon the movement his response pointer, so that he can apprehend immediately the relationship of contr movement to display movement. The subject can therefore copy the movement of th 'stimulus' pointer with his response pointer. And with predictable courses he can soo keep his response pointer more or less in line with the 'stimulus' pointer, by anticipatin its movements.

In contrast, in one-pointer balancing the subject is handicapped in two respects. Firs he can never see the direct effect of his control movements as such upon the movement the display pointer, for a variable 'stimulus' movement is always superimposed upo them. As a result he is slow to learn the exact relationship of the extent of contr movement to the extent of display movement. Even the directional relationship is n always clear to him initially. For whereas a quick control movement in an upwa direction may cause the display pointer to rise, a slow upward movement may not sufficient to counteract the falling of the display pointer caused by the 'stimulus' mov ment. Thus a slow upward control movement may appear to be moving the displ pointer slowly downwards.

The second handicap is the converse of the first. While the subject is responding cannot see the 'stimulus' movement uncomplicated by his response movements. On when he ceases responding for a while can he clearly apprehend it. For in order to obta information about the 'stimulus' movement while he is responding, he had to combi information from his control movement with the discrepancy revealed by the displ between this and the 'stimulus' movement. And the ability to combine information this way can only be learnt slowly and probably incompletely.

Thus, in two-pointer matching with predictable courses, the subject can soon copy a anticipate the 'stimulus' movement. Whereas before he can do this successfully in o pointer balancing, he has first to learn the control-display relationship; and subsequen to learn to abstract information about the 'stimulus' movement from his control mo ment and the visible error movement.

## VIII. Summary

1. Two-pointer matching was found to be about twice as accurate as one-poin balancing, over a wide range of course speeds.

2. After 2 hr. of spaced practice, two-pointer matching still held most of this le although the mean error had been considerably reduced in both cases.

3. The predictable simple harmonic course showed an initial advantage over the predictable more complex harmonic course in two-pointer matching, when the m

ors were expressed as proportions of the errors which would have occurred had the bject not responded at all. No such initial advantage was shown in one-pointer lancing.

4. After practice the predictable course showed this advantage with both displays.

5. An analysis of response times showed that the subject anticipated with both displays d both courses, right from the start of tracking. More anticipation was shown with the o-pointer display than with the one-pointer display. It was the simple harmonic course ich showed the most marked initial anticipation with the two-pointer display.

6. Practice increased anticipation. But as compared with two-pointer matching of e simple harmonic course, anticipation was never complete with the less predictable mplex harmonic course, nor with the more difficult one-pointer display.

7. Smoothness was found to be a less sensitive measure of anticipation. It showed nificant differences only with the predictable simple harmonic course.

8. It was suggested that the difference between the two kinds of display could be counted for by two advantages which the two-pointer display held over the one-pointer play: an uncomplicated view of the 'stimulus' movement, and a similar direct view of e effect of control movements.

9. Relatively small changes in the magnification of the display and of the required ntrol movements, did not significantly affect the accuracy of tracking with either type display.

I am greatly indebted to Prof. Sir Frederic Bartlett, F.R.S., for presenting the problems d theoretical background out of which these experiments have developed. Also to N. H. Mackworth, especially for assistance in report writing, and for pointing out the cessity of performing the learning curve experiment. The experimental subjects were pplied by the Royal Navy. I carried out the experiments while working as a member the Medical Research Council Applied Psychology Research Unit at Cambridge.

(*Manuscript received* 20 *November* 1951)

# PUBLICATIONS RECENTLY RECEIVED

*Scientific Session on the Physiological Teachings of Academician.* By I. P. Pavlov. 28 June–4 July 1950. Moscow: Foreign Languages Publishing House. 1951. Pp. 173. 2*s.* 6*d.* Distributed by Collet's Bookshops, London

This report of the speeches by several of the U.S.S.R.'s leading scientists provides a most interesting account of the physiological setting in relation to which Soviet psychology at present must try to develop. The speeches have one consistent theme—criticism of all anti-Pavlovian physiology and a strong plea for return to complete conformity with orthodox Pavlovian doctrine. Individual physiologists, such as Orbeli, who has carried out important research on the sympathetic nervous system, and Anokhin who introduced ideas of modifiability and self-correctability into conditioning theory, are severely criticized as 'heretical' and 'Sherringtonian'—epithets that are more or less synonymous in Soviet physiologic literature.

To psychologists, one particularly important point that emerges from the speeches is the vehement rejection of the 'subjective' method, even where this is used as complementary to physiologica 'objective' data. Here again it is the unfortunate Orbeli who bears the main brunt of criticism for h statement, in 1949, that 'we should aim at such a simultaneous study of the objective and subjecti processes as would permit us to trace their interrelation fully and to verify the laws of the activity of t nervous system discovered by both the objective and the subjective methods'. Orbeli is, in fact, accuse by Bykov of committing the supreme heresy of saying that Hering accomplished with his subjectiv psychological method what Pavlov only duplicated later with his objective method.

It seems therefore that introspection, even as a subsidiary method, is no longer considered desirab because it is 'idealistic' and 'anti-materialistic', while Freudian ideas and the whole psychosoma approach in medicine are rejected as being identified with the efforts of a reactionary bourgeoisie protect their own interests. Just how the bourgeoisie do this by psycho-analytical and psychosoma means is not made very clear.

Although the proceedings are directly concerned with physiological trends, the implications for t development of psychology in the U.S.S.R. seem only too clear. It is as though psychology in the W were to be put back to early Watsonian behaviourism, and a recent writer has suggested that t regression in the U.S.S.R. may be going even further, really consisting in a reinstatement of Bekhtere reflexology disguised in Pavlovian terms.

In the light of what we, from our 'idealistic' Western background, might discern as this dangerou reactionary trend in Soviet physiology, a recent review by I. D. London[1] of 'Contemporary psycholo in the Soviet Union' comes very opportunely. From this we find that, as was to be expected, the posit achievements for Soviet psychology are almost entirely concentrated in research on psycho-physiologi problems in conditioning, in sensation and perception. There is, for example, what appears to interesting and original work on 'sensory interaction', from which it has been established that access action of any number of sense modalities can affect the course of action of a given sense modality. Th auditory stimulation as a rule lowers the visual threshold for twilight vision. Further, Kravkov has fou that non-visual stimulation that increases retinal sensitivity to green light decreases it to red light, a vice versa. From this and other evidence the inference is drawn of the co-existence of two colo apprehending systems with opposed action (somewhat ironically, this would seem to support Heri opponent-colours theory).

Apart from this, however, the general situation for psychology that London describes seems, to the least, unsatisfactory, with the paralysing effect of Pavlovian orthodoxy discernible throughc Rubinshtein, the only Soviet psychologist to date to have developed a respectable general psychology paper, has to confess to the 'grievous sin' of only discussing questions in any way connected with Pav theory on six out of his 685 pages of text, and it is clear that 'the myth of Pavlov's all-round greati must be maintained at all costs', even where his contributions have been relatively unimportant as in application of conditioned reflex theory to psychiatry—a branch of medicine which he first serio began to study when he was 80.

[1] *Science*, vol. cxiv, 31 August 1951.

One must conclude from the speeches and from the wider psychological reference given to them by ndon's survey that the outlook in the U.S.S.R. for psychology as we know it is extremely grave. nes of development, such as Freudian psycho-analysis and Gestalt psychology, which in the West have en considered to be of great significance, are condemned as methodologically unsound and largely elevant to the materialistic requirements of contemporary Soviet research. Although this may be only emporary corrective phase in Soviet psychology, one cannot but feel that dictation by the 'master entist' of a research policy along strictly Pavlovian lines is unlikely to lead to anything but psycho- gical sterility. K. R. L. HALL

*volutionary Thought in America.* Edited by STOW PERSONS. Yale University Press (London, Geoffrey Cumberlege). 1952. Pp. x + 462. 32*s.* 6*d.*

his is an excellent book which should be in the library of every modern university. Its chapters were ginally given as lectures before an undergraduate conference at Princeton during 1945–6; they are the rk of scholars of the highest standing, writing so as to be understood without undue difficulty by any elligent undergraduate. We have heard a great deal recently in this country about the need to correct cessive specialism in university studies, but we have not heard to date of many positive and obviously itful suggestions as to how this should be done. The present volume shows us one way in which this o of countering excessive specialism has been done, and it has the merit of suggesting a number of other rtially parallel possibilities.

The first two chapters, by the philosophers Robert Scoon and F. S. C. Northrop, deal with the rise of olutionary ideas before Darwin, and with the effects of Darwin's researches and teachings on the general ea of evolution during the last half-century. A more balanced and reliable treatment than Scoon's, d a more stimulating one than Northrop's, could hardly be imagined: Northrop writes particularly ll of the evolutionary element in Aristotle's doctrine of 'form by privation', and of the relations and positions between this way of thinking and Hegelianism on the one hand and Darwinism on the other. en, something of an interlude, there is an 80-page essay, 'The genetic nature of differences among men', T. Dobzhensky, Professor of Zoology at Columbia. Here again, balance is combined with unusual cisiveness. So far as an outsider can judge, all the relevant evidence is surveyed and almost all the cially crucial points of application of genetic theory are considered. Dobzhensky's essay makes most hers I have read on this subject look pretty second-rate stuff.

Later chapters deal more specifically with 'Evolutionary thought in America'. Of particular interest readers of this *Journal* will be Edwin G. Boring's essay, 'The influence of evolutionary theory upon nerican psychological thought', a remarkably breezy performance. William James, we are told, was e anticipator rather than the founder of American psychology, its most characteristic earlier exponent ing Cattell. Other long and evidently important studies deal with the impact of evolutionary ideas American economics, American philosophy, and American political theory. And there are essays ich discuss evolutionary influences on literary and visual arts in America.

This volume should prove of the greatest value to all intelligent students of the social sciences in this untry. I hope also that it will be widely read by teachers of the social sciences here, and that it may mulate some of these to comparable efforts in effective intellectual co-operation. W. B. GALLIE

*Pensée en Orient.* By P. MASSON-OURSEL. Librarie Armand Colin. 1949. Pp. 216. 180 fr.

*Pensée en Orient* is an important work. So full and learned a treatise in the limited scope of a smallish ok might at first seem out of the question, but the author is a university lecturer of experience as well distinction. He has acquired the valuable habit of arranging his material so that it can be readily imilated by those less informed than himself, and it is well spiced with humour. He deals only with Near and Middle East and the immense influence of its thought upon the development chiefly of those ntries farther west which have, in the course of time, fallen under it. Most impressive is the handling he debt which Christianity owes to the Hebrews and of the effect of the impact of Hellenism after the quests of Alexander the Great. It is interesting to consider, for example, what would have happened Palestine if Judas Maccabeus had been less successful, or if Alexander had not died before his time. The summing up of the Christianity that emerged in the third century and the analysis of the influences urvived is very clear. The contribution made by Islam is given a far more just estimate than it usually eives at Christian hands, and one that explains why it is now to be found more powerfully operative he East of its source than to the West.

There is also something in this remarkable little volume for those who are looking for a justification of their beliefs rather than for a comparison of them and their origins with others, all of which have come out of Asia. This is the Pensée d'Orient in action. The author leaves us in no doubt that just as all religions which have acted as a basis for subsequent civilization found their beginnings in the East, so the spiritual and mystical attitude of the oriental thinker can influence religious thought in the West.

STELLA CHURCHILL

*Foundations of Inference in Natural Science.* By J. O. WISDOM. London: Methuen 1952. Pp. xvii+242. 22*s.* 6*d.*

This book repays study though it is not without defects. It has the interest of a rather heavily constructed detective story with a large number of suspects to be eliminated one by one. The dénouement occurs in the last chapter, as it should, with an appropriate degree of surprise, the latter being occasioned by the slightness of the culprit compared with the massiveness of his deeds. Nevertheless, one is left with a wish to know the culprit better, and with a belief in the consistent intellectual integrity of the author. There are no sly tricks.

The author presents a critical account of the more recent history of the theory of induction, dealing mainly with the views of Keynes, Broad and Popper, together with a useful summary of the probability theorems of Laplace, Bayes, Bernoulli, Poisson and Tchebysheff and, more recent, the views of von Wright, Reichenbach and von Mises. Dr Wisdom's own solution of 'the problem of induction' is first to declare that there's no such creature; secondly to replace it by the 'hypothetico-deductive method' associated with a view of probability sympathetic to both the 'frequency' concept and the 'range of possibilities' concept; and thirdly to justify the acceptance of an hypothesis which has not been falsified by contrasting it with *falsified* hypotheses, *so long as the universe is 'favourable'*. A universe is defined as 'favourable' if it has 'just enough order in it to render hypotheses reliable'. Dr Wisdom does not fail to recognize that his solution is a simple tautology, but he does fail to make explicit a vital feature of his own exposition which enables the tautology to be transcended. This feature resides in the two rather casual examples he provides, viz. drinking water on the hypothesis that it will allay thirst and shooting a dog to put it out of misery. *These are both actions not arguments.* In this reviewer's opinion probability and induction alike fail to escape tautology so long as they are conceived as relating to *propositions* alone. Seen as aspects of any *decision to act* they not only make sense but are recognized as daily necessities of human life.

The book cannot escape certain criticisms. First, the title is misleading. 'Foundations' are things on which we build. The word is misused by logicians to mean an abstract superstructure formed *subsequent* to the actual reasoning of scientists. Secondly, the word 'inference' in its strict *deductive* sense applies to a great deal of scientific reasoning. No account is given of this, the word being thus made to appear synonymous with 'induction', which is the real theme of the book. Thirdly, although the author begins promisingly by saying 'It is most important to begin by finding out what kind of inference scientists do use before proceeding to discuss the nature of their inferences', the few actual examples given are large somewhat trite text-book instances, atypical examples from that most atypical of sciences, viz. physics, the great bulk of his material consisting of *what logicians have said about science.* When will the methodologists really get down to a first-hand extensive and intensive study of what the scientists are actually doing, thinking and saying, instead of living by taking in one another's washing? They will find not only a far greater diversity of rational procedure than their categories can accommodate, but problems of a quite different order from those which they contemplate. A single example must suffice. In his *Autobiography*, Sir Arthur Keith summed up the Piltdown controversy thus: 'The real matter at issue was: Is there, or is there not a science of skull reconstruction?' Psychologists could provide methodological problems even more subtle and involved than this one. Perhaps there is an underlying fallacy in the very attempt to seek a monolithic methodology. The various sciences may embody such radically different approaches to nature as to require a plurality of logics.

G. P. MEREDITH

*On the Objective Study of Crowd Behaviour.* By L. S. PENROSE. London: H. K. Lewis and Co. Ltd. 1952. Pp. vi+74. Price 10*s.* net.

The title aptly marks out an important subject and would be appropriate as a collective name for many writings, past, present and expected, provided that the word 'crowd' were understood in a generalized sense. The members of Penrose's 'crowd' do not necessarily rub shoulders with their neighbours, but they are aware of belonging together: for example, the medical profession, when they are consider

ew drug, form one crowd; also the adult population of the United States, when they are electing their esident, form another crowd.

Even an objective study is undertaken with a conceptual background, and for a purpose. Penrose's ncepts include those of medical psychology and of statistics; and he aims to combine them in the ncept of the number of people who have the same idea, or the same potential reaction pattern. The derlying purpose is curative, in accordance with an extension of the Hippocratic Oath.

The book is small, being an essay, rather than a text-book or treatise. The subjects discussed include ting, crazes, outbreaks of religious enthusiasm, runs on banks and other panics, war, and the infectious perties of ideas. A particularly interesting example is the medical fashion for thallium-therapy, as own in Fig. 1 by the rise and fall in time of the number of articles describing treated cases and of the mber of critical papers.

Much attention is given to voting, as a restatement or development of Penrose's paper in the *Journal the Royal Statistical Society* for 1946. This part transcends the rest of the book in being provided with quantitative theory. Here the author has encountered the usual dilemma about mathematics in social ence, namely, that, if formulae appear, some interested readers will be perplexed, whereas, if proofs e omitted, other critical readers will pass by unconvinced. The compromise which he has adopted leaves veral features unclear. It is unfortunate that the first formula which the reader meets (p. 7, line 3) is viously incredible. If the reviewer understands Penrose's theory aright, it could suitably begin thus. ere is an assembly of $n$ voters, of whom $b$ form what Penrose calls the 'resolute bloc', and always vote gether. It should be noticed that the bloc is not assumed to be resolute in support of any ideological licy, but merely in voting together, so that the 'coherent bloc' would be a better name for them. The maining $n-b=r$, say, voters vote in a random manner. This might be because they have no opinion, because the proposals are exceptionally miscellaneous. Every vote is simply for or against, so that ere is a probability of $\frac{1}{2}$ that any random voter will vote either way. The probability that exactly $s$ of e random voters will vote the same way as the coherent bloc is

$$\frac{r!}{s!\,(r-s)!}\frac{1}{2^r}.$$

e sum of all such terms, taken over $s>\frac{1}{2}n-b$, is the probability that the bloc will be in the majority. hen $r$ is large, Penrose approximates, in the usual manner, to the sum of a run of these binomial terms way of a normal probability integral, the standard deviation being $\frac{1}{2}\sqrt{r}$. Thus did the reviewer find t that the fault in the aforesaid incredible formula is that a fraction-line has got into it by mistake.[1] correct statement would be that: the coherent bloc will be among the majority in about 841 votings t of 1000 when the number of voters in the bloc equals the square root of the number of random voters, ovided that the latter number is large.

Penrose makes a suggestion, which may be valuable, about voting in the United Nations Assembly. e deduces it from his theory, but the proposal may commend itself on other grounds. At present, to ote the Charter of U.N. (Ch. IV, Art. 18), 'Each Member of the General Assembly shall have one vote', that India has the same voting power as Iceland, although their populations are in the ratio of 2500 : 1. is is a monstrous absurdity. Penrose asserts that to make votes proportional to populations would be impropriety of the opposite sort, and that the proper compromise is to make votes proportional to the uare roots of populations. If so, India would have 50 times the vote of Iceland. The reviewer previously scussed voting power in an international assembly (*War and Peace*, Feb. 1918; *The World Outlook*, May 1926), and he concluded that the votes of any nation should be some index or measure of services ndered by that nation to the rest of the world, including foreign trade, loans of capital, and supply of eas. To see how nearly this resembles Penrose's proposal it is desirable to work out the correlation efficient between foreign trade and the square root of the population. L. F. RICHARDSON

*sychologie und Vernehmungstechnik bei Tatbestandsermittlungen. Vierte vermehrte und verbesserte Auflage.* By ALBERT HELLWIG. Stuttgart: Ferdinand Enke Verlag. 1951. Pp. xii + 355. 33 DM.

ie author, who died shortly before the publication of this edition, was one of the, not too numerous, minal court judges who combine an intelligent interest in the psychological problems of their work th a thorough knowledge of psychological literature as far as it has some direct bearing on those oblems. He was in a position, therefore, to take full advantage of his long experience in German criminal

[1] *Editorial Note.* Since this review was printed the point has been confirmed by an erratum slip.

courts and to produce, over a period of some forty years, a very considerable number of books and articl on the various aspects of forensic psychology. The book under review, first published in 1927, is one the best known and most important of his many publications. It is, of course, based on continental, mo specifically German, criminal procedure, which means that certain parts are not applicable to tria conducted according to English law (e.g. pp. 28 et seq., 254 et seq.). The book is valuable, however, f its discussion of certain basic problems of forensic psychology in the light of illustrative cases, take from the author's own court practice or from the literature. In Dr Hellwig's view these problems ha usually been treated too exclusively with reference to the witness, whereas the psychology of the accuse and of the examining magistrate or police official, etc., has been neglected, and he attempts to fill the gaps. Some of his most valuable chapters deal with confessions, especially wrong confessions, with t various sources of error in observation and remembering, with the value of the oath, with the specif problems of juveniles and of women, with suggestive questions, the use of hypnosis and narco-analysi The references, mainly to German, but also to French, Italian and American writings, have been brough up-to-date, but there is no bibliography, such as the admirable one provided in François Gorphe standard work *La Critique du Temoignage.* HERMANN MANNHEI

*An Experiment in the Prevention of Delinquency.* The Cambridge and Somerville Yout Study. By EDWIN POWERS and HELEN WITMER. New York :Columbia Universit Press. 1951. Pp. xliv + 649. $6.00.

Dr Richard C. Cabot, in writing on the successful treatment of delinquency, finds one necessary conditio that someone should come to know the man in so intimate and friendly a way that he comes to a bett understanding of himself and a truer comprehension of the world he lives in. This became the hypothes of a large-scale experiment involving many workers which was meant to cover a period of ten year War-time conditions made necessary certain alterations including the length of time for which it w continued. The first part of the report is by Edwin Powers and deals with the nature of the experime and the methods used. In the second part Helen Witmer discusses the results and conclusions. These a precisely what one would expect when the friendly understanding given to the boys who take part com mostly from people who are not qualified to deal with more serious cases of maladjustment.

The study had to find its own subjects, and these were people who had not sought out help for t difficulties from which they suffered. Schools therefore were used as a starting point, and from the referrals and a few others, several judges selected a number of pre-delinquent boys. The fact that the boys might be neurotic or prepsychotic but not necessarily predelinquent was not considered. Fro these boys carefully matched control and treatment groups were formed, the boys in the treatment grou being allocated to counsellors who were responsible for work done with them. Such work was oft inadequate and superficial as the report has to admit. Moreover, the nature of the experiment made impossible for the conditions of the control group to be kept rigid.

At the end of the experiment the overall results showed no difference between the control and trea ment groups. More detailed analysis showed, however, that the study had worked successfully wi some boys where the maladjustment was mild and manifested inwardly and the home conditio favourable. In some other cases which were more difficult there may have been some harm done by t counsellors.

Some chapters deal with the judgements of the boys on the work of the study, and this is compar with the results obtained for such boys. These and the chapters presenting some of the cases are use and even interesting. Tables and statistics, and also the questionnaire and rating forms, are presented length. Nevertheless, the report is disappointing, and it might be advisable to confine one's reading the very comprehensive foreword by Gordon Allport, since the book is very lengthy and contains mu needless repetition. A. J. LA

*Trends in Psycho-Analysis.* By MARJORIE BRIERLEY. London: The Hogarth Press Lt and The Institute of Psycho-analysis. 1951. Pp. 320. 21*s.* net.

Dr Brierley sets herself the formidable task of convincing psycho-analysts that research and the scienti attitude are not bad objects. She is remarkably persuasive in her differentiation of the clinical from t research method, of perceptual from conceptual thinking, of personology from that unnecessary a inaccurate concept metapsychology; but particularly interesting is the fact that she feels it necessary be so persuasive—'whether we like it or not, we all have research responsibilities. We evade these unl

e pay due attention to the theoretical implications of our own data....' Research is, however, quite nocuous. 'There is nothing intrinsically mysterious or alarming about theory which is only the system probable explanations.'

This book may be of particular interest to psychologists in that the writer believes that once psycho-nalysts 'have arrived at working hypotheses regarding the principles of type differentiation, they will e well advised to compare and collate their results with those of experimental psychologists, social sychologists and sociologists'. Indeed, her colleagues are informed that they should not 'rule out the ossibility' of obtaining help from the comparative study of instincts advocated by C. S. Myers. One els, however, that the comparing and collating might be attempted before 'the completion of their own oundwork'. References to psychological work are, in fact, sketchy and fashionable and by no means ie most relevant available. Nevertheless, Dr Brierley has made a courageous attempt to widen the utlook of psycho-analysts and to increase their tolerance. Her chapter on the work of Melanie Klein, r example, is a sanguine corrective to the earlier choleric attack by Glover. Instinct theory, we are told, by no means complete or final, next to nothing is known about empathy. 'Indeed, it becomes in-reasingly evident that every aspect of analytic theory calls for re-examination in the light of process-oncepts.' Psycho-analysis should no longer be identified with Freud. Views such as these will certainly e welcomed by non-analysts.

A chapter on 'Trends in psycho-analysis' outlines changes in theory with regard, for instance, to arcissism and aggression, and a warning is given against regarding libidinal development as a means to ie mastery of anxiety connected with aggression, for it is upon the interaction of libido and aggression iat development depends. Less fortunate is the remark that 'We are particularly interested in the oidinal and aggressive components of oral impulses, but we ought not to forget that they contain unger-components too'. Even Casanova did not find it necessary to remind us that we sometimes go to ed to sleep.

Almost half of the book is taken up by the final chapter on 'Psycho-analysis and integrative living'. ptimal integration is taken as the goal of personality development. The Christian mode of integration, is argued, is based upon an alliance of reality-self and conscience against instinct and infantile selves, par-cularly against infantile eroticism and aggressiveness. This clearly leads to the idealization-denigration olarity which is so unstable. Psycho-analysis, on the other hand, aims at the development of a higher egree of psychological reality-sense and ego-toleration of hitherto intolerable components of the psyche. his argument, though not perhaps new, is well worth repetition and restatement, particularly when resented with such fairness and skill. G. A. F.

*sychanalyse et conception spiritualiste de l'homme.* By Joseph Nuttin. Publications Universitaires de Louvain. 1950. Pp. 436. 140 fr.b.

is not easy for the English reader to formulate to himself exactly what the author has in mind when he peaks of 'une psychologie spiritualiste'. The term has no reference to 'spiritualism', nor has it any irectly religious connotation. Since the very title of the work suggests a certain opposition or contrast etween 'psychanalyse' and 'psychologie spiritualiste', one is reminded of Roland Dalbiez's characteriza-on of Freud's work as 'the most profound analysis that history has ever known of the less human ements in human nature'. And this gives us the clue to Prof. Nuttin's thought, for on p. 7 he tells us iat by 'psychologie spiritualiste' he understands a psychology which 'sets into relief that which is ecifically human in man's activities'.

The author points out that hitherto, in the eyes of many, modern therapeutic psychology has remained mewhat of a closed system, having the minimum of contact with, and dependence on, the rest of the eld of psychological science. In his view, the findings of depth psychology need to be brought into lation with, and supplemented by, general psychology, in order that a body of knowledge adequately escriptive of the normal individual may emerge. Such an adequate description, he believes, must take ccount of psychic activities at three levels: (*a*) that of individual needs and propensities; (*b*) that of the dividual's relation to the social body; and (*c*) that of his relation to the transcendent.

In his examination of current psychological theory and practice, Prof. Nuttin's critical treatment lies ot so much in rejection and opposition, as in pointing out the necessity of enlarging the field by the clusion of results already authenticated and accepted from the sphere of non-medical psychology. point of special interest lies in his insistence on the fact that not all psychological conflict should be ought of as morbid. There are many 'tensions constructives' as he calls them, which are a necessary ondition for healthy dynamic progress.

This admirable work will be especially useful to those critically minded readers who wish to see modern therapeutic psychology take its rightful place in the onward march of psychological science as a whole. There is an extensive bibliography, referring to works in several languages, and an excellent index.

J. LEYCESTER KING, S.J.

*The Psychology of Pierre Janet.* By ELTON MAYO. London: Routledge and Kegan Paul Ltd. 1952. Pp. x + 132. 8*s*. 6*d*. net.

This is a small book giving an introduction to the study of hysteria and obsessional neuroses based on Janet's psychology and the author's own experience and outlook. It is clearly and sympathetically written, and deals with the problems of neurosis in a superficial but helpful way, and is suitable as an introduction to these problems for elementary students. The book will lead the thoughtful reader to a perusal of the existing translations into English of Janet's own works.

R. W. P.

*Essentials of Behaviour.* By C. L. HULL. New Haven: Yale University Press. 1951. London: Geoffrey Cumberlege. 1951. Pp. vii + 145. 18*s*.

In this book Prof. Hull brings together the revisions of his basic postulates and their corollaries which have been made necessary by experimental work carried out since his *Principles of Behaviour* appeared in 1943. Most of the twenty-nine brief 'chapters' are crisp, succinct statements describing a postulate or corollary, together with an indication of the evidence on which it is based.

At the outset Prof. Hull describes his theory of behaviour as 'molar', but it is important to note that he uses this word in a special sense, different, for example, from the sense in which it is used by Tolman. According to Hull, molar theory yields a less perfect behavioural science than might be possible by means of a more developed neurophysiology, and there is nothing which molar theory can teach us which neurophysiology could not, in principle, do better. According to Tolman, molar behaviour has properties which would be completely left out in any neurophysiological account of behaviour. For Hull, molar and molecular units differ in degree; for Tolman, they differ in kind.

About half of the book is taken up with a consideration of the factors entering into reaction potential. Five such factors are discussed, viz. drive, stimulus intensity, incentive, the gradient of delay in reinforcement, and habit strength. One new factor, stimulus intensity, has been introduced, and one, temporal asynchronism of conditioned to unconditioned stimulus, has been omitted. No claim is made that this list necessarily exhausts the possible factors constituting reaction potential.

In a book limited to essentials an author cannot be expected to demolish his critics. Nevertheless, one would like to know, for example, how Prof. Hull proposes to reconcile his views with the recent work of Meyer at the Wisconsin Primate Laboratory, which suggests that the primary drives, such as hunger, may be of comparatively little importance in learning. Harlow's suggestion that possibly 'monkeys are not familiar with modern motivational theory' at least provides food for thought. Again, the obvious shortcomings of the 'wat' as a unit for measuring reaction potential, of which Prof. Hull himself is fully aware, are not likely to remain disregarded. But whatever major or minor changes may have to be made in Hull's system, one must pay homage to its coherence and remarkable flexibility, not less than to the rigour of its mathematical construction.

The student interested in the detailed development of the system is referred by its author to 'twenty-five volumes of handwritten notebooks' compiled during the past thirty-seven years and deposited in the Yale University Library.

JOHN COHEN

*Asymmetries of the Visual Space.* By MARTTI TAKALA. Helsinki: Suomalaisen Kirjallisuuden Seuran Kirjapainon Oy. 1951. Pp. 175. 500 mk.

This monograph reports briefly some eighty experiments concerning the influence on perception of the position of objects in the visual field. It provides abundant confirmation and extension of those asymmetries or anisotropies with which the Gestalt psychologists have made us familiar. The chief conclusions are: The main dimensions (the horizontal, vertical and sagittal) are 'preferred', as the framework of perception, as compared with oblique positions. In a normal articulated environment, containing familiar objects, the horizontal is preferred to the vertical; but the more the field is 'reduced' the more the horizontal seems to lose its preference. The main dimensions, moreover, are themselves asymmetrical so that preferred main directions are found within them. Thus in the perception of stationary objects the

irections 'to the left', 'upwards' and 'towards' are predominant. Again, the left and upper halves of ıe visual field are preferred to the right and lower halves. There is not space to review the author's etailed illustration of these general principles, but his experiments appear to have been conducted with ıre and ingenuity, and while some of the results are, as he says, only preliminary, the monograph will ɔrm a valuable guide for further research.

The author favours a 'central' theory of these phenomena. He is more cautious, however, than the estalt psychologists, and in place of speculations concerning a neural field, he suggests the possible nportance of motor functions. Thus he provides evidence that handedness is correlated with asym-ietries; the asymmetry is often reversed in the left-handed, and also, as a rule, weaker. In conclusion, one ould like to see further examination of the author's statement that two-dimensional perception is in ɛneral preferred to three-dimensional. Greater attention might also be paid perhaps to the influence of ıe position of the observer's head. The results might be considerably different with the head at an angle ɔ the upright, or inverted, and not in the normal position as in these experiments. The translation is ɛnerally clear and smooth.

R. B. JOYNSON

*he Annual Review of Psychology*. Edited by CALVIN P. STONE and DONALD W. TAYLOR. Volume 3. Stanford, California: Annual Reviews, Inc. 1952. Pp. ix+462. $6.00

his volume of the *Annual Review* contains the following articles: *Child Psychology*, by Nowlis and Nowlis; *earning*, by Harlow; *Vision*, by Helson; *Hearing*, by Garner; *Somaesthesis and the Chemical Senses*, by Vendt; *Individual Differences*, by Humphreys; *Personality*, by Eysenck; *Social Psychology and Group rocesses*, by M. B. Smith; *Industrial Psychology*, by Brown and Ghiselli; *Comparative and Physiological rocesses*, by Nissen and Semmes; *Abnormalities of Behaviour*, by Zubin; *Clinical Methods: Psycho-iagnosis*, by Magaret; *Clinical Methods: Psychotherapy*, by Raimy; *Counseling*, by Gilbert; *Educational sychology*, by Elmgren; *Statistical Theory and Research Design*, by McNemar; and *Motivation*, by lowrer.

These articles are intended to cover recent work, published mainly in the previous year, on their articular topics. They somewhat resemble, therefore, the reviews published at irregular intervals in the *sychological Bulletin*. But because they cover a briefer period of time, they tend to centre about the ıost popular subjects of experiment, and to neglect the background and general setting given by earlier ork. Moreover, if the author is to cover the bulk of the experimental work published during the period nder review, he has little space to devote to a critical appraisal or to general theoretical aspects. Some uthors, notably Eysenck and Mowrer, have been more selective, and have concentrated on what appears ɔ them the most important publications. Their articles are in consequence more interesting and com-rehensible to the general reader. But at the same time they have omitted material which might be aluable to the specialist who is looking for everything relevant to his particular line of study. Helson onsiders that it is not fair to confine a review only to the highlights of the field; the author must 'attempt ɔ give an over-all picture while including as much detail as possible', and must bear in mind the non-ɔecialist as well as the specialist.

A further consequence of the necessity of concentrating on what is fashionable at the moment is that ertain topics are omitted, while others are discussed in several articles, some publications appearing gain and again. Thus the popular topics of testing and test procedures are reviewed in eight or nine of ıe seventeen articles; they dominate those on *Psychodiagnosis*, *Counseling*, *Educational* and *Industrial sychology*. It seems that these techniques may have what one author designates as 'an almost insidious ppeal to gadget- and gimmick-minded American psychologists'. Rat behaviour and attitudes to social roups are also popular; and the article on *Social Psychology and Group Processes* is one of the best attempts ɔ give a coherent interrelationship to a wide field of work. By contrast, reasoning and thinking get only ½ pages altogether, in Harlow's article on learning; motivation is treated by Mowrer almost entirely as an spect of learning; and emotion is scarcely mentioned (it does not appear in the index). This avoidance of ıe basic problems of motivation and their relationship within the personality is most unfortunate; it leaves ur concept of the personality with a hollow centre, inadequately filled by a hypothesized 'ego'. It was pity also that Prof. Piaget was unable to prepare the review on *Child Psychology* since he might have been ble to discuss the fundamental processes of development, in addition to superficial behavioural aspects.

In fact, this volume is probably of most value to those whose specialized interests coincide with the ıshionable topics of the present day. It is perhaps idle to upbraid it with failure to give a balanced ɛview of every aspect of psychological work.

M. D. V.

*Fundamentals of General Psychology.* By JOHN FREDERICK DASHIELL. Third edition. London: Pitman. 1950. Pp. xii+690. 27*s.* 6*d.*

This text-book, intended for elementary students, has been revised to include 'intensive cultivations in well-established areas' subsequent to 1937. It deals with the physiological basis of behaviour, general psychological processes, statistical methods, personality and some areas of genetic, social and industrial psychology. The author has quite obviously gone to considerable pains to include current research in these numerous areas, and in its up-to-dateness the text is to be commended. But the scope is rather too wide for a book of this size to deal adequately with all these fields in either their contemporary or more traditional aspects. Much of what has been included might well have been either omitted or considerably expanded. For example, Sheldon's work on physique and temperament is discussed briefly, and the reader learns too little about either its content or the ways in which it is a development of, and possibly beyond, the earlier research of Kretschmer. What might be regarded as areas of development of some importance, the systematic learning theory of Hull and the opposing theory of Tolman, are each dismissed in a single paragraph. More theoretically neutral subjects like attention, transfer of training, statistical methods, test construction and test validation are dealt with in sections that do not compare well with those of alternative texts. The chapter on intelligence gives a picture of unanimous acceptance of Spearman's general factor theory that may mislead the student reader. This, together with a rather naïve identification of '*g*' with 'general ability', reads strangely in a chapter that includes major works by Thomson and by Thurstone in its bibliography. More satisfactory parts of the book, for instance, the chapters on thinking and on language, reveal a competent and experienced psychologist whose thought is often of a most stimulating kind. Again, the book is rather outstandingly well illustrated, the illustrations being exceedingly well annotated. The style achieves the aim of making the text readable 'without compromise to scientific soundness'. A pervading emphasis on scientific method is valuable in a book designed for the first-year student. While useful for supplementary reading, because of its comprehensive inclusiveness, this text-book loses a great deal from its tendency to deal much too briefly with what it has been decided to include. P. MCK.

*The Psychology of Character.* By A. A. ROBACK. London: Routledge and Kegan Paul Ltd. Third edition, revised and enlarged. 1952. Pp. xxx+747. 42*s.* net.

Though emanating from Cambridge, Mass., *The Psychology of Character*, first published in 1927, is in the German rather than the American tradition. It is very long; and some four-fifths of its many pages are devoted to an historico-critical survey, in which a gallant attempt is made to mention everything that has been said about character from Theophrastus to Eysenck. Considerable space is devoted to such topics as phrenology and the doctrine of 'humours'.

In the last part of the book the author makes his own contribution to the theory of character. He defines character as 'an enduring psychophysical disposition to inhibit instinctive impulses in accordance with a regulative principle' (p. 568). The last six words are important. 'The core of character' is inhibition *based on principle*; inhibition deriving from self-interest or social motives does not count. 'The man of high character (and there is just as much reason for talking about high character as about a high intelligence quotient) is exemplified by the Roman father who sentenced his fiendish son to death, thus inhibiting the paternal instinct in deference to the principle of justice. Firmness is the quality which typifies character at its best' (p. 572).

Clearly 'character', in the sense defined, must not be equated with virtue. A spontaneously benevolent person, who has little need to control his impulses, will be a good man, but he will not necessarily be a man of high character, in Dr Roback's sense. Dr Roback fully accepts this conclusion, and urges that the term 'character' must be freed from its ethical implications if it is to be of use to psychology. He argues this point forcibly, but his transparent admiration for character makes it difficult at times for him to live up to his own principles; value-judgements will keep breaking in.

To avoid raising false hopes, one should perhaps mention that the author is quite uninterested in the physiological basis of character. He conscientiously devotes two chapters to the subject, but they are almost comically inadequate and out of date. MARGARET KNIGHT.

*Survey of Rewards and Punishments in Schools.* By M. E. Highfield and A. Pinsent. National Foundation for Educational Research in England and Wales, Publication No. 3. London: Newnes Educational Publishing Co. Ltd. 1951. Pp. xxiv+432. Price £2. 2*s*. 0*d*.

his long-awaited report will disappoint many readers, for though it provides a very sane and balanced scussion of the extremely difficult topic of the use of corporal punishment in State schools, and though presents full details of very extensive investigations of teachers' opinions (and, to a much more limited tent, pupils' opinions), it has very little fresh to say, particularly on the psychology of incentives and terrents. The persons responsible for planning the research did not regard any actual experimentation feasible. Instead, the authors claim to have used 'operational research', though one would question e applicability of this term to data consisting exclusively of impressions, ratings and attitude atements. However, the investigations were carried out in a most thorough manner on large and presentative samples, and the results were submitted to expert statistical analysis.

The surveys of the history and present state of regulations regarding punishment in schools, and of the aits of 'difficult' children, are of little interest. More important psychologically is a study of the reported fectiveness of eleven positive incentives and twelve deterrents in controlling troublesome behaviour. his shows that the more severe deterrents, such as corporal punishment, are the least used but are nerally considered the most efficacious; also that teachers' views of the relative effects of various centives differ from those of pupils. The latter clearly regard experience of successful achievement as ore satisfying than praise or the conferral of positions of authority. This study reveals, too, a number of teresting differences in the effects of incentives or deterrents on different types of children, and different asses of misdemeanour.

The authors recognize the arguments in favour of a more psychological approach to difficult children an corporal punishment, and describe thirteen schools where such punishment had been virtually andoned. These schools are not untypical: for example, they include a secondary modern boys' school a poor urban area. Their success appears to depend on (*a*) competent staff, (*b*) generally progressive aching methods, (*c*) reasonable co-operation from the homes. Nevertheless, eleven of these thirteen ad-teachers, together with the majority of teachers in general, favour the retention of corporal punishent, to be used as a last resort for certain social and moral offences. Opinions differ widely, and women, achers in rural schools, and younger teachers are on the whole less favourable to it than men, urban, d older teachers. The final summing-up brings out clearly the complexity of factors involved, and shows y there is no simple, single solution.

While it is valuable to have all the data published, the book is somewhat indigestible and somewhat pensive. There would be a good case for a shorter summary, which might circulate widely among the aching profession, many of whom clearly hold much more extreme pro- or anti-views than the average sults might indicate. P. E. V.

*e Envelope: A Study of the Impact of the World upon the Child.* By James S. Plant. New York: The Commonwealth Fund; London: Geoffrey Cumberlege. 1951. Pp. x+299. 24*s*. net.

e 'envelope' is the late Dr Plant's name for the processes by which the individual selects from the vironment those features with which he can deal and shuts out the rest, and this book examines pects of the child's development in an environment which he finds only partly acceptable and manage-e. It reports impressions that Dr Plant gathered during twenty-five years' psychiatric work as director an American clinic for delinquent or difficult children, though the problems he discusses are the ordinary blems of any children, to which he often brings fresh insight. He gives, for instance, a valuable account insecurity and inadequacy, noting their close interrelation and their production of similar symptoms, showing the important differences in the treatment they require. His discussion of sexual experience soundly based on the distinction between direct sexual satisfactions and those derived from the ondary meanings that sexual activity may have for the child, such as approximation to adult behaviour, cess in competition, being valued and adequate; he notes, for instance, the strong appeal of sexual erests for the adolescent second-rater, who can find compensation in this field for poor success else-ere. Dr Plant's good sense does not preclude sensitive insight, as he shows for instance in discussing ie feeling of difference', and noting the association between the sense of being an individual and the se of guilt.

Although not disciplined by numerical data and statistical analysis, Dr Plant's book has the more intimate discipline achieved by a scientific mind with no axe to grind and no career to be made. He is cool in reporting disagreement with his views, and he cheerfully indicates the limits both of his observations and of his understanding. In contrast with many psychological writers he gives the impression of harmony between his emotional response to the subject-matter and his intellectual study of it, neither being sacrificed to the other.

*Occupational Choice.* By ELI GINSBERG, SOL. W. GINSBERG, SIDNEY AXELRAD and JOHN L. HERMA. New York: Columbia University Press; London: Geoffrey Cumberlege. 1951. Pp. viii+271. 63*s*.

This book sets out to give a tentative theory of the way in which occupational choices come to be made, the data being provided partly by previous research and partly by free interviews with boys and young men ranging in age from 11 years to the early twenties. In order to simplify the issues, the subjects were deliberately selected for the absence of overt intellectual, physical or emotional handicaps and from a socio-economic level which minimized material obstacles to their choice of work. For purposes of comparison, supplementary data were also collected from boys of a lower economic level and from young women.

In its attention to the *un*handicapped, in what it has to say about phantasy, tentative and realistic choices, and in its suggestions for further research, the book has merits. But as a whole it is disappointing. First, the presentation leaves much too hazy the relation of the authors' theorizing to their original data. Extracts from young people's histories and comments are freely quoted, but the reader is always in the dark about how many young people in each group illustrated the tendency discussed. Secondly, the theory which emerges is not a *new* mouse. The gist of it amounts to the following: (1) 'occupational choice is a developmental process: it is not a single decision, but a series of decisions made over a period of years'; (2) the process, though capable of modification in detail, 'is largely irreversible', in that later decisions are limited by previous decisions; (3) 'the process ends in a compromise'. Perhaps it is unfair to remark that this elderly animal has been the 'familiar' of British vocational psychologists for thirty years, for the authors are clearly fighting the still too general and facile assumption that an individual 'worth his salt' can overcome all barriers. But it is constructive to ask whether the word 'compromise' was well chosen. In this context it suggests not (as the authors intend) 'make the most of the ingredients and if one combination is not possible try another' but 'make do', and to the young that is apt to mean the end of zest.

J. W. REEVE

*Graphology: A Handbook.* By HENRY A. RAND. Cambridge, Mass.: Sci-Art Publishers. 1947. Pp. 200. $3.00.

*The Hand in Psychological Diagnosis.* By CHARLOTTE WOLFF. London: Methuen. 1951. Pp. xv+218. 32*s*. 6*d*. net.

Similarity of intention and in subject-matter would appear to make consideration of the above two books in a single review appropriate. One might be tempted to add as a further unifying factor the suspicion in which such techniques are held by a proportion, if not a majority, of psychologists. There is, however, little scope for comparison, or attempt at synthesis, since the approach, style and general outlook of the respective authors could hardly be more different.

Rand's *Graphology* claims to be both 'a valuable reference work for the more advanced student' and 'a sure guide for the average intelligent person'. The principles of graphological analysis and interpretation are fairly enough stated and illustrated by numerous handwriting specimens; unfortunately these are rather poorly reproduced. An attempt is made to link graphological signs with a system of psychological trait analysis. The author's 'trait-list', however, appears to be an *ad hoc* production unrelated to any form of systematic psychological theory, and there is a good deal of confusion between different conceptual levels. This, rather than the entire absence of validation data, renders any claim to scientific status for the book negligible. Such a claim, however, is advanced by Dr A. A. Roback in a lively and polemical Introduction, the force of which is somewhat diminished by over-protestation.

Charlotte Wolff's book is a totally different proposition. It is a sequel to the author's earlier work *The Human Hand*, and embodies the results of more recent research, on the basis of which she has found it necessary to modify some of the opinions previously expressed. Nevertheless, one is still referred

e earlier book for a full exposition of the author's 'Method of Hand Interpretation'. Dr Wolff's approach as much in common with that of Sheldon, from whose work she seems to have received considerable spiration. Although the book, like Rand's, is partly addressed to 'every intelligent educated person', is rather weighed down by technical language, particularly from the field of endocrinology. Very full search data are quoted in support of the interpretation method, and these, in the absence of first-hand perience, cannot fail to impress the reader.

In contrast, the underlying psychological assumptions are less convincing. Thus, Dr Wolff writes: 'The radial portion of the right hand is the most important tool of conscious activity and it is understandable from this that the quality of the intelligence has a special correlation with this part of the hand. he whole left hand as well as the ulnar part of the right have a different significance. The left is the assive side and not only in regard to the hand but to the whole body.... One can assume that the whole ft side of the body and the left hand in particular, are linked with the emotional and imaginative side the personality and therefore with the subconscious mind. The ulnar part of the left hand ought to be pecially significant in this respect as it is the least used for action. The same holds good to a minor degree r the ulnar zone of the right hand.'

It is in the impossibility of validation of this type of functional symbolism that the inherent weakness work such as Dr Wolff's lies, and this irrespective of the statistical evidence for differences between oups formed on some principle deriving from such assumptions. To this reviewer, at least, even the ore rarefied graphological 'signs' carry more conviction. B. S.

*his Matter of Mind.* By Brian H. Kirman. Thrift Books, No. 16. London: C. A. Watts and Co. Ltd. 1952. Pp. 93. 1*s.* 0*d.*

he author of this book, who is a psychiatrist, is concerned to prove that a mentally healthy and peaceful ciety can be produced by rational egalitarian reform of social conditions. The influence of hereditary ctors in determining intelligence and ability he considers to be relatively unimportant; and he summarily smisses theories of innate instinctive disposition and of the forces of the Freudian 'unconscious'. He lieves that the development of psychology will help us to understand ourselves and to control the world tuation. It seems unlikely that his book will assist this development.